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THE PSYCHOLOGICAL REVIEW

THE NET RESULT OF THE ANTI-HEREDITY MOVEMENT IN PSYCHOLOGY

BY ZING YANG KUO

Shanghai, China

INTRODUCTION

The controversy on the question of instinct and heredity in psychology was started some seven years ago. At first it was merely an attempt to show that the concept of instinct had been greatly abused in psychology, and that some restriction of its usage was necessary. The usefulness of the concept of instinct in the social sciences was also questioned. Then, there appeared some articles purporting to deny the existence of instincts. This was followed by the attempt on the part of some of the instinct deniers to remove the entire concept of heredity from psychology and the controversy began to assume a broader form than the mere question of instinct. While the whole issue should be finally decided by future experimentation, it seems opportune to sum up critically the salient features of the controversy and to point out the directions toward which the problems of instinct and heredity have been shifted through recent discussions. In this paper I shall try to examine some of the more important contributions to the problem during recent years and to state my final view on the matter. I shall deal with only the more essential points brought forth by recent writers on both sides of the controversy. But as this article is not in the nature of a general review of the recent literature on the subject, no attempt will be made to include in our discussion every

writer who has participated in one way or another in the instinct or heredity controversy.

THE ATTACK OF THE SOCIAL PSYCHOLOGISTS ON INSTINCT

The anti-heredity movement has been participated in by two groups of writers, the social psychologists and the behaviorists. It may be said at the outset that the interest and methods of procedure of the social psychologists in attacking instincts are different from those of the behaviorists, and hence their results are not the same. While the social psychologists attack the problem of instinct from the standpoint of its applicability to social sciences, the behaviorists are interested in it purely as a laboratory problem.¹

Among the social psychologists who have participated in the attack on the concept of instinct are Allport, Ayers, Bernard, Dunlap, Faris, Josey, Kantor and others. While these writers differ in details, they seem to agree on the following points in their objection to the concept of instinct.

1. The concept of instinct has been misused and abused. This charge every psychologist, even including McDougall, will admit. But the charge does no harm to the concept of instinct whatsoever. If it has been abused, the remedy is to exercise more care in using it; it does not necessitate the abolition of the concept.

2. Classifications of instincts are always arbitrary. This is also true. But to argue that as the classification of instincts is arbitrary, therefore there are no instincts, is to employ false logic. For it is obvious that the arbitrariness of classifying instincts is no evidence against the existence of instincts.

3. The animistic conception of instinct, a concept that defines instinct in terms of inner forces, is unacceptable on

¹ Personally I do not have the slightest interest in such subjects as social psychology. I doubt very much that such a science can be established without assuming some over-individual mind or over-individual behavior. If social psychology deals merely with the responses of the individual to social stimulation, as some recent writers have claimed, it belongs to the domain of the general science of human behavior. For, except the movements of the fetus and of the very young infant, practically every human behavior is social in nature. If social psychology studies the social behavior of the individual, what then, will be left for general psychology?

scientific grounds. But does this point help the case? For note the so-called mechanistic view of instinct which defines instinct in terms of the nervous system has no better scientific ground than the animistic one.²

4. Practically all the asserted human instincts are, in the last analysis, acquired habits. The present writer in his earlier writings on the subject also made the same point.³ This seems to be the great point made by the instinct attackers. But is the point well taken? It must be remembered that this point is based on the sharp distinction between learned and unlearned reactions. Such a distinction is as unsound as the concept of instinct itself.⁴ Indeed, to deny instinct without at the same time rejecting the concept of habit is self-contradictory,⁵ as we shall see more clearly later on.⁶ If the instinct deniers show that the asserted human instincts are acquired habits, the instinct psychologists will demand, with equal justification that the instinct deniers explain those experimental results which have demonstrated unlearnedness in certain activities. In other words, only by accepting the validity of the distinction between the learned and unlearned responses, can the social psychologists deny the entire concept of instinct. Furthermore, most of the leading instinct psychologists have repeatedly asserted that there are no pure instincts in human adults, that is, most of the human instincts are overlapped, modified or superimposed by habits so that it is very difficult to find any pure instincts in human adults. Now if the instinct deniers find no instincts in human adults, it does not weaken the position of the instinct psychologists, for few of them have ever

² See Kuo, Z. Y., 'A Psychology without Heredity,' *Psychol. Rev.*, 1924, 31, 427-448; also Carmichael, L., 'Heredity and Environment,' *J. Abn. & Soc. Psychol.* 1925, 20, 245-260.

³ Kuo, Z. Y., 'Giving up Instincts in Psychology,' *J. Phil.*, 1921, 18, 645-664; also, 'How are our Instincts Acquired?', *Psychol. Rev.*, 1922, 29, 344-365.

⁴ See my 'A Psychology without Heredity,' *loc. cit.*

⁵ See Woodworth, R. S., 'A Justification of the Concept of Instinct,' *J. Abn. & Soc. Psychol.*, 1927, 22, 3-7.

⁶ Dunlap seems to be the only one among the instinct attackers of the social psychology group who does not accept the distinction between the learned and the unlearned or between instinct and habit. See his article on 'The Identity of Instinct and Habit,' *J. Phil.*, 1922, 19, 85-94.

insisted that adult human behavior is instinctive. Indeed we have made our attacks on the instincts from a wrong angle. From the very start, we accepted the validity of the concept of heredity in psychology and emphasized the antithesis between the inherited and the acquired. We were thus driven to the absurdity of defining instincts in terms of smaller units and relegating them to infants and animals. As long as animals and infants possess instincts, no matter how simple they may be, the attempt to retain the concept of instinct is justified. As a matter of fact, the social psychologists have not actually denied instincts at all, they simply reduced the instincts to smaller units (this is also true of my own immature articles already referred to). They have accomplished their purpose in showing that the acceptance of instinct is valueless for the social sciences and for social psychology, but they have left the concepts of instinct and heredity in psychology as unvitiated as before. Moreover, in their reconstruction of social psychology, they have substituted for instinct something just as objectionable as the instinct concept itself, namely, the concept of habit. Dunlap's substitution of desires for instincts makes the case even worse. In fact, judging from Dunlap's conception of desire one will be inclined to think that Dunlap's objection to instinct is simply because the instinct concept is not mysterious enough, that is, not so mysterious as to make it 'conscious' and 'introspectively observable.'⁷ Indeed, those who have acquired a morbid phobia about 'consciousness' and 'introspection' will certainly think that the concept of instinct is really better.

5. These social psychologists not only accepted the validity of the concept of heredity in general, but also emphasized the importance of the inheritance of the so-called 'mental' or behavior traits. Dunlap's 'Social Psychology' serves as the best example.

6. That most of the social psychologists who have participated in the anti-instinct movement are not interested in the instincts as a laboratory problem cannot be denied.

⁷ See Dunlap, K., 'Instinct and Desire,' *J. Abn. & Soc. Psychol.*, 1925, 20, 170-173.

Their writings are really of what Watson has called the arm-chair variety. Indeed, if these social psychologists ever looked to the laboratory for the solution of the instinct problem, they would not have been so bold as to assert that only infants and animals possess instincts. As a matter of fact, the behavior of animals is much more complex and variable and much less stereotyped than they have imagined at their writing desks. In my recent studies on the behavior of the cat towards the rat, which will soon be reported in the *Journal of Comparative Psychology*, I have come to the conclusion that neither the concept of instinct nor that of learning nor both together can explain the development of the cat's behavior. We will recur to this matter at the end of this paper. I am merely interested here in pointing out that the problems of instinct and heredity can never be solved by armchair speculations.

7. It is curious to note that in spite of their vigorous attack on the concept of instinct, many of these social psychologists have accepted the notion of inherited nervous disposition without any question. Bernard's conception of heredity is just this sort of thing. I have elsewhere pointed out that the neurological speculations concerning instinct are ungrounded. I shall try in a later section of the paper to make my point clearer.

8. More curious still is the fact that the instinct attackers of the social psychology group have, either explicitly or implicitly, followed the instinct psychologists in accepting the preformation theory. Carmichael has criticized Bernard and Allport in that, in spite of their denial of instinct, their own conceptions of heredity are just as preformistic as the concept of instinct.⁸ I think the criticism is well founded. It will be unfair, however, if we make the same charge against Josey, who, to my mind, is the most thorough-going and most consistent among the instinct deniers of the social psychology group.⁹

In brief, my objection to this group of writers is that they

⁸ Carmichael, L., *op. cit.*

⁹ See Josey's 'Social Philosophy of Instinct.'

are half-hearted, inconsistent, and somewhat insincere in their denial of instincts, that they simply try to push the concept of instinct outside the domain of social psychology and the social sciences without making any further attempt to solve the problem, and that they have paid too little attention to the laboratory side of the problem, being content with pure armchair speculations which have always been the chief characteristic of the instinct psychologists. Now, let us turn to the behaviorists and see how they look at the problem.

THE ATTITUDE OF THE BEHAVIORISTS

For more than ten years the behaviorists were busily engaged in their quarrel with the mentalists over the questions of consciousness, thinking and introspection. They seemed to be so fully occupied that neither time nor energy was left for them to critically examine the concepts of instinct, heredity, and trial and error. Furthermore, these concepts have been in vogue among the animal workers. And behaviorism is a product of animal psychology. The result is obvious. The behaviorists accept instinct, heredity, habit and trial and error *in toto* without questioning their validity. Thus, we find that such topics as instinct, emotion, and habit occupied the greater part of Watson's earlier books. Even as late as 1924 Watson made no attempt to correct his views on instincts, emotions and habit, when he revised his 'Psychology from the Standpoint of a Behaviorist.' This is also true of the earlier publications of Lashley, M. Meyer and Weiss.¹⁰

However, after the anti-instinct movement had been started, practically all behaviorists began more or less to realize the necessity of revising their former views on the

¹⁰ In a personal communication Professor Warren informed me that Dr. E. B. Holt rejected the concept of instinct long before any anti-instinctivistic article had appeared. But, unfortunately, so far Dr. Holt has not seen fit to publish his views, although, judging from his comments on my articles through correspondence, I am reasonably sure that he must be classified among the radical instinct deniers. (Professor H. S. Langfeld, in his article on 'Consciousness and Motor Response' has informed us of a forth-coming book by Dr. Holt on 'Animal Drive, Instinct and the Learning Process.' But as these lines are written, I have not been able to ascertain whether or not Dr. Holt's book has appeared.)

matter. The change in Watson's attitude toward instinct is most spectacular and somewhat radical. Since Watson has made a most vigorous attack on instinct during the last two or three years, we may be justified in confining our present discussion to this author alone.

There can be no manner of doubt that Watson has attacked the problem of instinct purely from the laboratory standpoint. He has come to the conclusion that there are no instincts because he could not find them in the nursery. While I am in full agreement with Watson in several aspects of his recent view on heredity, especially his analogy of instinct with the boomerang,¹¹ I feel compelled to part company with him on the following points:

1. While denying the existence of instincts, Watson still retains the distinction between the learned and the unlearned. Such a retention will make Watson's position very difficult. Are not the instinctive actions always considered as unlearned actions? If there are no instincts, on what ground, then, can we classify responses into the learned and the unlearned? In fact, Watson has accepted the traditional criterion of unlearnedness to measure the existence or non-existence of instincts. Thus, after he has failed to discover any of the asserted human instincts in the nursery, he comes to deny instincts. But if the instinct psychologists can demonstrate to Watson (and I am sure that they can) that there are certain types of reactions which can be performed without learning, will he not be forced to declare that certain instincts do exist? As a matter of fact, no instinct psychologist will insist that every instinct appears in infancy. They will point out to Watson that there are many delayed instincts which appear rather late in human life and therefore are not easy to be detected, due to modification, or to the overlapping and superimposing of habits. Indeed, the failure of the nursery to discover instincts does not preclude the possibility of their appearance in later life. The instinct psychologists may even go so far as to say that even in infancy habits and instincts are already mixed together, and

¹¹ Watson, J. B., 'Behaviorism,' pp. 78-79.

that it is difficult, if not impossible, to distinguish one from the other.¹²

2. That Watson is still loath to give up the whole concept of heredity is clearly evidenced by his two chapters on emotion in his 'Behaviorism.' While in his 'Psychology from the Standpoint of a Behaviorist,' emotion is defined as an hereditary mode of pattern reactions, in the 'Behaviorism' the word 'hereditary' is purposively avoided. But does it improve Watson's position to substitute for heredity such expressions as 'What Emotions Are We Born With?' or 'The Unlearned Emotional Equipment.' Unless they are metaphysical entities, I do not think that the so-called 'emotions' can 'be born with' or 'acquired.' I even do not agree that the behaviorists are justified in retaining the concept of 'emotion.' Men and animals, under stressing situations, may display chaotic and sometimes violent movements. This, of course, we all admit. But they are the direct results of the stressing situations. They are not emotions either in the mentalistic sense or in the sense of pattern reaction. Nor can they be born with, nor acquired. They are what they are because of the stressing situations, because such situations fail to elicit well organized and well regulated responses. If we were to use the word 'pattern' in this connection, at all, the responses under stressing situations are really characterized by lack of definite patterns. Watson may reply that what he means by emotions is just these chaotic and sometimes violent movements or sometimes momentary paralysis of overt movements. But then, he must not speak of them as pattern reactions. Nor should he include such responses as what he has called 'love,' 'jealousy,' 'attitudes,' etc., under the head of emotions; for, beside the vagueness of these terms, they do not belong to the category of chaotic or disorderly or disrupted responses. And, moreover, he should cease to speak any more of inborn or original or hereditary or acquired emotions. Certainly it is a curious inconsistency

¹² In an article on 'The Behaviorist Looks at Instinct' (*Harp. Mag.*, 1927, 155, 228-235), Watson has been driven to the absurdity of accepting, as did the social psychologists, instincts in animals. This is not to be wondered at in view of Watson's acceptance of unlearnedness as a valid criterion of instincts.

for Watson to admit the inborn or original emotions as against the acquired ones, while denying the inheritance of instincts and 'mental traits.'

3. As I have elsewhere pointed out, one cannot consistently deny instinct and heredity without, at the same time, giving up the concept of habit in its traditional sense. Apparently Watson has committed this error.

My quarrel with Watson is that he has hesitated, that he is not thoroughgoing enough in his denial of heredity, and that he seems to be oscillating and somewhat confused in his treatment of instincts, emotions and habits.

Throughout this paper I have criticized practically every author connected with the anti-instinct or anti-heredity movement. I do not mean to imply that I myself have been free from the above criticisms. In fact, in my first two articles on instincts ('Giving up Instincts in Psychology' and 'How Are our Instincts Acquired?'), I practically took the same position as that now held by Watson and the social psychologists. To be specific, in these two articles, I still accepted the validity of the concept of heredity in psychology as well as the antithesis between the inherited and the acquired. I repudiated the concept of instinct chiefly because the current instincts were, in the last analysis, acquired habits. Like the social psychologists, I reinterpreted instincts in terms of smaller units ('units of reaction' or 'random movements'). I also unscrutinizingly accepted the concepts of purpose, drives, motive, tendency, habit, and trial and error. These are the specific and indefensible flaws I now see in the two articles. Through a more careful reading of biological literature, through more critical thinking, and particularly through my experiments with the cats, I have come to a realization that my former attempt to repudiate instincts in psychology was half-way and inconsistent. The article on 'A Psychology without Heredity' was, in part, intended to remedy some of the defects of my former writings. But the present paper will represent more clearly my present view on the matter.

THE DEFENSE OF THE CONCEPT OF INSTINCT

Let us now turn to those psychologists who have come to the defense of instinct and heredity during the last few years. Here we come to a very curious fact. The defenders of instinct and heredity have contributed more than did the attackers to the complete breakdown of the concepts of heredity and of instinct in psychology. That is to say that these psychologists have tried to defend instincts by first denying them. Thus, Hocking, McDougall, Tolman and Wells have come strongly to the defense of instincts by ringing a death knell to the traditional view of instincts based as it was on physiological assumptions. McDougall would cast out all instincts as they are ordinarily understood by biologists and psychologists of the Thorndike and Watson school and substitute for them some very genuine ones, instincts that are always unrefutable because they are never scientifically approachable. In a similar way, Professor Tolman gives 'the pure reflex pattern theory its final *coup de grace*'¹³ and asserts that instincts must be interpreted in teleological rather than in physiological terms. But as I have elsewhere¹⁴ shown and shall later show, Professor Tolman's objective view of purpose is no better than, or is even inferior to, McDougall's straight-forward animism. Hocking's position is essentially the same as that of McDougall in that they both try to rescue instincts from the attack of the instinct deniers by taking them into the custody of a metaphysician of the Bergsonian school. Wells admits all the faults of the instinct concept which have been pointed out by the instinct deniers, but insists that instinct must be redefined. And he redefines it in terms of 'normal environment.'¹⁵ But the concept of instinct as redefined by Wells is no longer the same as that which the biologists and psychologists have been used to understand.

¹³ Tolman, E. C., 'The Nature of Instinct,' *PSYCHOL. BULL.*, 1923, 20, 201-202.

¹⁴ Kuo, Z. Y., 'The Fundamental Error of the Concept of Purpose and the Trial and Error Fallacy,' *PSYCHOL. REV.*, 1928, 35, 414-433.

¹⁵ Wells, W. R., 'The Meaning of Inherited and Acquired in Reference to Instinct,' *J. Abn. & Soc. Psychol.*, 1922, 17, 153-161; also 'The Anti-Instinct Fallacy,' *PSYCHOL. REV.*, 1923, 30, 228-234.

Thus, instincts have been defined not by giving actual evidences but by changing definitions. This clearly shows that instinct has not been a fact, but merely a subject for book and article writing. Perhaps this is the most outstanding result of the whole anti-heredity movement. Perhaps this is the result which instinct deniers who have attacked the problem from the laboratory standpoint can best expect. For if the question of instinct (as well as of heredity) is no longer a question of fact but merely a matter of opinion, as the defenses of the instinct psychologists have most clearly shown, the laboratory student may just as well go ahead with his experimental researches without bothering himself with the mere opinions of psycho-metaphysicians.

Other psychologists who have defended the concepts of instinct and heredity have thought that without these concepts behavior cannot be explained. Why do we behave as we do? Why does not the rat behave like the cat? All these questions, so the argument goes, must be answered by such assumptions as instinct and heredity. I do not think that science has to answer these questions at all. I mean that no science should go beyond the descriptive level. Specific stimuli determine specific responses; given a stimulus, a definite response can be predicated. What else do we need besides this for the scientific description of behavior? Those psychologists who always demand abstract principles for the explanation of behavior may profit by taking a lesson from Watson's boomerang. Certainly no modern physical scientists need to assume instinct and heredity in order to explain why electrons, atoms or molecules behave as they do.

Woodworth has recently tried to justify the concept of instinct by showing that since we cannot repudiate reflex and habits, we must keep instinct as a check or foil to habit.¹⁶ Woodworth seems to think that we are unable to cast out the concept of habit. But this is not true. In my experimental researches as well as in constructing a behavioristic system (which will be first published in the Chinese language) I have found that the concept of habit is just as useless to me as that of instinct; in fact, I have found that no single

¹⁶ Woodworth, *op. cit.*

concept or nomenclature of the traditional psychology is of any use for the scientific description of behavior.

As to the concept of reflex, of course, no one can deny that reflex is a concrete physiological fact. But one who still insists that the reflex mechanism is hereditary pure and simple may be referred to such works as those of Child, Detwiler, Kappers and others in order to appreciate the importance of environmental factors in the development of the nervous system.

There are, however, certain psychologists who in recent years have tried to defend the concept of instinct by demonstrating certain types of activities which can be performed without learning. Such facts demand a more serious attention from the experimental standpoint. We shall discuss the question of unlearnedness more fully in a separate section.

VITALISM OR PREFORMATION?

Through recent discussions on the questions of instinct and heredity in psychology it has become clear that the believers in instinct and heredity are either preformists or vitalists. In going over again and again the literature on the subject I have found that not a single instinct psychologist can escape from this dilemma. If we consider the problems of instinct and heredity as experimental problems, I do not think that we need to go into the discussion of the problem of vitalism at all. Vitalism has its place in metaphysics. But it can hardly demand any serious attention from men of science.

The preformation view of heredity as entertained by the contemporary psychologists really belongs to the Roux-Weismann variety. It is essentially a biological theory of the last century, being outgrown, on the one hand, by the more modern form of preformation as advocated by T. H. Morgan and others, and on the other, being completely repudiated by the contemporary epigeneticists such as C. M. Child and others. The antiquity and crudity of the preformation theory in psychology have been clearly pointed out in a series of articles by Dr. Carmichael.¹⁷ It is not necessary

¹⁷ Carmichael, L., *op. cit.* See also his two articles on 'Study of the Development of Behavior in Vertebrates Experimentally Removed from the Influence of External Stimulation,' *PSYCHOL. REV.*, 1926, 33, pp. 51-58, and 1927, 34, pp. 34-47.

for me to discuss this matter any further. I merely want to mention the fact that this crude psychological preformationism is based on a morphological point of view, hence before the theory can claim any serious attention at all, the psychologists must first show that the so-called hereditary responses are reducible to definite and localizable morphological terms. But this is almost impossible for the psychological preformist to do, as I have shown in my article on 'A Psychology without Heredity.' Thus, the theory of psychological preformation is virtually reduced to mere verbal abstraction besides being outgrown by the more modern biological theories.

Psychological preformationism is already dead in view of the recent advances in developmental physiology and in biological theories of development and heredity and in view of the failure of the psychologist to reduce behavior to definite and fixed morphological facts. And the only course which is left for us to take, if we are still unwilling to abandon the concept of heredity in our science, is vitalism. Vitalism or preformation? Or rather vitalism or no heredity? Can any psychologist avoid this dilemma? Those who are not willing to accept McDougall's hormic theory, certainly cannot make any other choice.

IS A THIRD ALTERNATIVE POSSIBLE?

In making the above statement I am fully aware of the recent attempts made by several writers to steer a middle course. We have seen the attempt made by Professor Wells to redefine heredity in terms of normal environment. We have also seen Professor Tolman's attempt to objectify the concept of purpose so that instincts may still be retained. Wells' redefinition is really nothing more than a 'verbal trick,' as Dr. Carmichael has clearly pointed out.¹⁸ As to Professor Tolman's behavioristic view of purpose, I should say that it is really McDougall's animism under disguise. If Professor Tolman merely infers from animal behavior that the animal is purposive without reference to the internal

¹⁸ Carmichael, L., *op. cit.*

striving of the animal, the question will immediately arise: what kind of purpose is it? Is not Dr. Tolman's inference that the animal's behavior indicates purpose a mere analogy derived from human introspection. Does not the statement that the animal is seeking for some goal imply that the animal in question is 'consciously striving' for some end? In other words, Dr. Tolman's behavioristic view of purpose is a true animism plus intolerance, that is, he implicitly assumes purposive striving in the mentalistic sense but does not allow us to speak of it in mental or introspective terms. Indeed, Dr. Tolman's theory is intolerant in practice, as well as inadequate in method of procedure; it is inferior to the method of introspection. For by introspection the purpose can be directly experienced, while by inference from behavior, it can only be indirectly assumed; the reliability of such an assumption can never be tested, since we are not allowed to use introspection. Of course, Dr. Tolman may reply that what he means by purpose is not a mental phenomenon but may be reduced to some physiological facts such as nervous disposition or motor set. I have discussed this motor set concept of purpose more fully in another article on 'The Fundamental Error of the Concept of Purpose and the Trial and Error Fallacy.' To avoid unnecessary lengthening of the present paper I will not restate my arguments already presented there.

Let us now turn to a more important proposal to maintain a middle course in the heredity controversy, namely the proposal of Dr. Carmichael. In a series of articles already referred to, Dr. Carmichael refuted the psychological preformationism and its derivative, the hypothesis of 'mere maturation' or 'magic birth.' He says that these theories are not in harmony with more recent views of heredity and development in biology. Thus far, Dr. Carmichael seems to be in complete agreement with me. But here, he pauses and finally refuses to travel with me any further on the same path. He protests against my attempt to dismiss the concept of heredity from psychology. He believes that heredity and environment are not antithetical nor separable, but inter-

dependent. He says that every response is just as hereditary as it is acquired. If I understand Dr. Carmichael correctly, he seems to mean that every response *implies* hereditary potentiality. Without hereditary potentiality no response can be actualized. Therefore, he argues, in understanding behavior, the concept of heredity is indispensable. I need not go so far as to deny the concept of hereditary potentialities in biology. As long as the biochemist has not been able to produce living protoplasts of different species from test tubes, some sort of hereditary potentialities will have to be assumed in order to explain why 'like produce like.' But this is purely a biological problem. Whether we need such an assumption in the science of behavior is an entirely different question. Personally I do not think that in our behavior study we need the concept of hereditary potentialities at all. As I have already pointed out, except very simple reflexes, no response can be reduced to definite, fixed and localizable morphological facts. We must remember that in heredity we deal with definite, concrete and localizable morphological structures. When the Mendelian workers attack an hereditary problem, they point to definite bodily characters. But this is almost impossible for us to do in psychology since behavior is so variable, and has no definite bodily characters or definite and fixed neuromuscular patterns. For this reason I recommend that the problem of heredity be ignored in our behavior study. The Mendelians deal with definite bodily characters, hence they can study heredity in mathematical terms. On the other hand, since in behavior we do not deal with definite and fixed neuromuscular patterns, any study intended to measure either qualitative or quantitative differences in the hereditary factors of behavior is out of question. *Heredity must always be concerned with definite physio-morphological facts. Otherwise, it is beyond any experimental approach, and will always remain as an unverifiable abstraction which serves no scientific purpose whatever.*

Carmichael says that in all hereditary maturation there is learning, while in all learning there is hereditary maturation. From this he concludes that the antithesis between instinct

and habit must be abolished. Carmichael here seems to reach a wrong conclusion although he has started from a right premise. If it is assumed that hereditary factors are present in every acquired response, and that every acquired response is an actualization of hereditary factors, our conclusion should not be that the antithesis between instinct and habit be abolished, but that both of these concepts must be cast out altogether. For, note, *since hereditary factors are present in every response, and since the qualitative and quantitative differences in heredity between two responses cannot be measured, we will ask: Of what use is the concept of heredity in our behavior study?* In other words, heredity cannot serve as a *differentia* in behavior study, hence it does no harm for us to ignore it in the science of behavior. The student of behavior ignores heredity as he will ignore gravity. That the factor of gravity is present in every response no one can deny. But since its influence is both qualitatively and quantitatively constant and equal in every behavior (except in some very rare cases), it cannot be a basis upon which responses can be differentiated, and we are justified in not taking gravity into account when we investigate behavior. In short, the argument for a psychology without heredity is not necessarily that heredity has nothing to do with behavior but rather that it is not experimentally approachable and that it cannot be a behavior *differentia*.

Heredity is concerned with definite physio-morphological problems. It is fundamentally a biological problem. But it is negligible in the study of behavior. We can accept the organism as given and start to investigate its behavior in response to environmental stimulation without reference to heredity. We need not ask how the organism comes about, or how heredity determines the organismic pattern. This is a question the biologist must answer. When the biologist delivers us an organism of a given species in a given stage of development, our duty is to find out how and what stimuli can effectively force this organism to behave and in what manner it behaves. Behavior is not a manifestation of hereditary factors, nor can it be expressed in terms of heredity;

it is the direct result of environmental stimulation. Behavior is not inherited, nor is it acquired. It is a passive and forced movement mechanically and solely determined by the structural pattern of the organism and the nature of the environmental forces.

UNLEARNEDNESS AGAIN

Perhaps the chief difficulty for most of us in getting over the concept of heredity lies in the fact that we do not seem to be able to explain the apparent facts of unlearned behavior without reference to heredity. To those experimentalists, such as Chas. Bird, L. Carmichael, C. P. Stone and others who have demonstrated that there are certain types of behavior which can be performed without learning under controlled conditions, our attempt to make a psychology without heredity seems to be a flight from reality. Indeed, if a psychology without heredity is based on the assumption that all responses are postnatally acquired, the work of these experimentalists should be sufficient to discredit the whole assumption.¹⁹ Fortunately, however, our attempt to dismiss the concept of heredity from psychology has nothing to do with this assumption. It is rather based on the view that the two concepts of heredity and learning, of instinct and habit, *whether they are considered as antithetical or as interdependent*, can no longer be used as behavior categories.

Our objections to the use of unlearnedness as a criterion of inheritance may be summarized as follows:

1. Strictly speaking, except the first movement of the fertilized egg, there is no real unlearned response. Every response is determined partly by the present stimulation and partly by the past history of behavior of the organism. While there may be apparent cases of behavior which can be performed without learning, the indirect influence of the past experiences of the organism on the so-called unlearned behavior cannot be denied. If we can use the term 'learning'

¹⁹ In my recent observations of the behavior of pigeons, I found that there are several types of activity which were performed without learning, but not being satisfied with using the mere verbal concept of heredity or instinct to interpret these observations, I am now endeavoring to discover the exact factors which are responsible for the unlearned responses observed.

at all, it must be conceded that learning takes place immediately after fertilization as many recent embryological works seem to have shown. And if we accept the view (as all of us must accept) that every action in response to stimulation has its effect directly or indirectly upon subsequent responses, we could hardly conceive of any purely unlearned behavior in any stage of development.

2. The concept of unlearnedness is based on two false theories, namely, (1) the theory of maturation, (2) the theory of trial and error. In the first theory it is assumed that the neural pathways for the unlearned response is preformed, their synaptic resistance is low so that upon mere maturation they can function without learning, and that, on the other hand, the neural pathways for the acquired behavior must be formed, and synaptic resistance reduced, by practice. Carmichael has shown that recent discoveries of developmental physiology have definitely given discredit to the maturation theory. Indeed, if the development of the nervous system is the result of living functioning, the result of the excitation-response processes between the organism and its environment, the concept of 'mere' maturation of neural pathways becomes meaningless. As to the formation of new pathways in so-called habit-formation, Watson²⁰ has pointed out many theoretical difficulties for the concept. Besides, Ulrich's work²¹ and especially that of Lashley²² indicate that the formation of new pathways does not seem to take place in the so-called learning. Moreover, Lashley's work has also made the concept of the reduction of synaptic resistance through practice untenable. The fallacy of the trial and error concept has been fully discussed in another article.²³

3. I have already pointed out in my article on 'Giving up

²⁰ Watson, J. B., 'Psychology from the Standpoint of a Behaviorist,' p. 293.

²¹ Ulrich, J. L., 'Integration of Movements in Learning in the Albino Rat,' *Psychobiol.*, 1920, 2, 375-447, 455-500, and *J. of Comp. Psychol.*, 1921, 1, 1-95, 155-199, 221-286.

²² Lashley, K. S., 'Studies of Cerebral Function in Learning,' *PSYCHOL. REV.*, 1924, 31, 369-375; also *Arch. Neur. & Psychiat.*, 1924.

²³ Kuo, Z. Y., 'The Fundamental Error of the Concept of Purpose and the Trial and Error Fallacy,' *PSYCHOL. REV.*, 1928, 35, 414-433.

Instincts in Psychology' that while a complex act may be performed without learning its component acts are previously acquired reactions.

4. Unlearned behavior can be better explained by other factors than by heredity. This has been discussed in the article on 'How are our Instincts Acquired'?

5. In the article on 'A Psychology without Heredity' I have pointed out that the distinction between learned and unlearned responses is too crude to be of any use for experimental purpose.

In conclusion, let me mention again my observation concerning the behavior of the cat toward the rat. I have found that some cats kill rats without learning, but some will have to 'learn' it by so-called 'imitation,' others 'learn' it by accident. I have also found that cats could be made to 'fear' the rat. But it may even 'love' and protect the rat. Indeed, so variable is the cat's behavior toward the rat that neither the concept of heredity nor that of learning, nor those of instinct and habit, can be used as adequate explanations. The cat can both 'instinctively' kill and 'instinctively' 'love' and protect the rat, but it can also 'learn' to kill and fear the rat.

A psychology without heredity is a psychology which proposes to do away with not only the concepts of heredity and instinct but also all their related concepts such as habit, trial and error, imitation, insight and purpose. It proposes to study behavior as concrete actualities and refuses to be muddled up with any abstract and teleological concepts. Its view is essentially passivistic in that it considers every action as a 'forced' response to be described solely in terms of the functioning of the environmental stimulation. Indeed, unless we take such a view as this, I do not see how we can explain the conflicting results of my observations on the cat's behavior toward the rat.

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THE METHODS OF KURT LEWIN IN THE PSYCHOLOGY OF ACTION AND AFFECTION

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With the translation into English of books by Professor Köhler¹ and Professor Koffka² and the publication of papers by them in American journals,³ the American psychologist has had sufficient opportunity to become acquainted with some aspects of Gestalt psychology. The papers of Helson⁴ have also greatly helped to further the chief tenets of this school among us. At the same time there are several fields of Gestalt investigation almost unknown in America today. The purpose of this paper is to call attention to the work being done under the direction of Professor Kurt Lewin in the fields of action and affection. From the standpoint of the Gestalt psychologists very interesting results are being obtained, only a part of which have as yet been published.⁵

The experimental psychologist, particularly in the field of

¹ 'The Mentality of Apes,' New York, 1925.

² 'The Growth of the Mind,' New York, 1925.

³ 'The Psychologies of 1925,' Clark University publication.

⁴ 'The Psychology of Gestalt,' *Amer. J. Psychol.*, 1925, 36, 342-370, 494-526; 1926, 37, 25-62, 189-223. Also printed in assembled form.

⁵ This paper is concerned chiefly with 'Untersuchungen zur etc.' (Titles 1-4 of the appended bibliography). A complete bibliography of Lewin's psychological papers is, however, appended for those who wish to trace Lewin's work leading up to these problems.

The writer, who was working in the Berlin laboratory while most of the investigations were at an experimental stage, has been able to obtain the proof sheets or the manuscripts of such as have not yet been published, and has thus through the kindness of Professor Lewin been enabled to include the most recent material in his survey. Such experiments as are reported without definite reference are taken from manuscripts or proof sheets and cannot be referred to directly.

The plan of this paper will be to outline Lewin's methods and concepts as developed on his program papers (1), (2) and show how these methods are working out in concrete experimental problems. It will be necessary to follow the argumentation of the program papers rather closely, developing in some detail the ideas to which Lewin gives much careful and thorough analysis.

the higher mental processes has up till now been inclined not to insist upon an absolute validity for his psychical laws. He has tended rather to a set of laws which are qualitatively limited, open to constant exception. Let the subject have a headache, let his attention wander, or his emotional balance be upset, and the law no longer applies in the particular instance. We have scarcely ever had laws as the scientist understands laws, but rather statistical regularities. Lewin's first point is that we must look for laws that hold under all conditions, both quantitatively and qualitatively. We must firmly restate the possibility of absolute laws in psychology. By so doing we can put the psychological experiment on a firmer basis and greatly increase its possibilities.

In the last generation the psychologist, thus dealing merely with statistical regularities, chose as his criterion of the successful experiment, verification through repetition.⁶ In sciences which are based on true laws, experimentation is not limited to finding repeatable experiments. A single individual case suffices to prove or disprove an hypothesis providing that the conditions governing this case are sufficiently controlled. Moreover such a limitation obliged the psychologist to deal largely with situations far removed from everyday experience. What we really are, or should be, interested in psychologically is not the exceptional but rather the everyday phenomenal experience. Emphasis of the extraordinary which has persisted into the psychology of today is a remnant of a psychology that was more a collection of curiosities than a systematic science.

Another characteristic of psychology of the past was its essential atomism. All former experimentation on will, action and affection was done in an attempt to find the elements at work in such processes. When found they were put together additively as so many mere parts or items of an accidental whole. We know now that many acts are of a unitary nature and the first task of the experimenter is to see if the act he is dealing with is of such a unitary or Gestalt type.

⁶ For a more thorough discussion of the scientific theoretical aspects see 'Gesetz' etc. (5).

To do this he must turn from the microscopic to the macroscopic type of experimentation. In the act of writing, the response shows certain definite Gestalt qualities in the way the lines are formed, the rhythm in placing the letters on the page, and so forth; an attempt to reduce writing to $a + b + c$ —different microscopic movements—loses the essential in the process. From the standpoint of Gestalt theory the total activity, as well as the surrounding field in which it is embedded has to be taken into consideration.⁷ Equally important moreover is the make-up of the inner or psychic field. A person attempting to copy a fancy Spencerian handwriting and then sitting down and writing off a letter to a friend is performing two completely different acts. Writing a letter is not writing in the former sense at all. The actual hand movements are more like the movements of the mouth in speaking, something merely accessory to the real act—*i.e.* the imparting of information. The motor components here represent no independent moment in the total act. They are embedded rather in the whole psychic act of imparting information. These psychic processes are also usually of a Gestalt nature.

Other complications have to be considered. Similar results by no means imply similar activities in achieving them. Performances where practice plays a part, can by no means be considered as in every case identical. Typewriting, to give an instance, demands from the beginner and from the finished typist two completely different types of motor action. The actual seeking of letters by the beginner and the touch method of the adept are different single moments in the whole process. The same achievement, then, does not imply the same psychical process.

Lewin now comes to an important distinction which must be made in considering behavior. Psychologists in the past were constantly confusing the phenomenal and the conditional-genetic sides of acts. Two acts can be phenotypically quite different and genotypically identical—or the reverse.

⁷ The term 'Umfeld,' 'surrounding field,' is used by the Gestalt theorists to denote this setting of outward conditions and larger activities, without a knowledge of which we cannot determine the significance of an act.

Thus an embarrassed child may exhibit its shyness in blushes and confusion, or it may become loud-voiced and assertive—the two modes of behavior are genotypical equivalents. On the other hand the play-acting of emotion and real emotion may be said to resemble each other phenotypically, although genotypically quite opposite.⁸

In any science real laws are descriptions of the conditional genetic type. That is, a law is simply a description of a genotype. Former psychologists have confused the phenomenal with the conditional-genetic in that they have used externalities, mere adhesions, as causal factors. The contiguity of *a* and *b* was the supposed cause of *a* reproducing *b* in the basic law of association. The same holds for the coupling together of the conditioned and the unconditioned stimulus in the conditioning process. The fact that the unconditioned becomes adequate is by no means a causal explanation for any response. There must be definite sources of energy to refer back to. Lewin finds these in psychic tensions which govern the whole act in the sense of energetics. Such a tension striving towards discharge supplies the energy for, and is thus the cause of the behavior. In the intensity of the stimulus itself is to be found no source of energy. For this the stimulus is inadequate. The energy which will cause a starving man to quicken his footsteps at sight of a loaf of bread, to seize it and devour it, is clearly not to be looked for in the loaf of bread itself. On the other hand without the loaf of bread there would have been no such quickening of the footsteps, no such display of energy. The function of the stimulus is to influence and control the energy supplied by the tension. The manner in which it may do this is three-fold. It may cause a tension to be set up, it may bring an already existing tension into play, or thirdly, it may control the motor action by acquiring a stimulating character, positive or negative (*positive oder negative Aufforderungskarakter*). When a stimulus is of this character it has the function of directing or steering the

⁸ Lewin goes somewhat more fully into the concepts of genotype and phenotype for all the sciences in 'Gesetz' etc. (5).

action in the field of behavior. When the particular action is fulfilled, the tension is discharged and the psychic energy returns to a state of equilibrium. An example will make this clear. A child, absorbed in its play, suddenly espies a piece of chocolate on the table out of its reach. A tension is induced by the perception of the chocolate which acquires a positively stimulating character.⁹ The child puts its hand out for it but cannot reach it. The table is too high. Its actions, as we see, are directed entirely by the forces in the field of behavior (*Feldkräfte*). The stimulus is the chief of these, but other forces, obstacles, act as barriers which prevent its reaching it directly. It runs *away* from the table, tries to attract its mother's attention, fails, finally drags a chair across the room, climbs on to it, and in this way reaches the desired piece of chocolate. Follows a reduction of the tension and a return of the system to equilibrium. A moment later the child may perceive its Teddy Bear or some other toy, and the whole story will start over again until this tension too is discharged and equilibrium is again restored.

Thus we see that psychic tensions, like physical ones, tend to come to a state of equilibrium. This tendency need only be exhibited by the system as a whole. Parts of the system can progress towards a heightening of the tension in those parts, but the final stage is always equilibrium.¹⁰ Then again tensions become stable and lose—phenomenologically, that is—their tendency to discharge. Wishes and half-finished acts represent tensions of this type. But as soon as the proper situation arises, fulfillment of the wish will occur and the unfinished act will be resumed. We will meet definite experimental evidence for these views later on.

On first view this theorizing resembles certain behavioristic concepts in American psychology. Such resemblance is,

⁹ The case of the starving man and the loaf may be taken as an example of the second way in which a stimulus may function. The tension, due to hunger, is already in existence, and is brought into play by the perception of the stimulus, in this case the loaf of bread.

¹⁰ That such systems exist purely physically has been shown by Köhler, 'Die physischen Gestalten,' Erlangen 1924. Cf. also v. Bertalanfy, 'Die Bedeutungen, etc.,' *Biol. Zent.*, 1927, 47.

however, only superficial. In the first place Lewin is making no attempt to establish a physiological correlation. The tensions are not physiological tensions. Whether or not they have physiological parallels or are ultimately to be referred back to the physiology of the organism are not questions which here interest him. He is dealing with purely psychic tensions, operating in a definite psychical field.¹¹ The tendency within this field towards a reduction of tension and to a state of equilibrium is the cause of behavior. Moreover, the reaction, instead of being a mere mechanical response to the stimulus, is governed by all the forces in the field of behavior. It is dependent—and here we see the Gestalt pattern—on the field forces and on the changing field relationships between subject and stimulus, here let us say between the child and its candy. Indeed, once a child is 'satiated' and has had as much candy as it can eat, ten pieces of chocolate in the 'surrounding field' will have no power to induce a new tension; the chocolate has lost its stimulating character. It may even in fact develop a negatively stimulating character, and what before attracted may now repel.¹²

Lewin we see is here introducing a causal dynamic factor which is missing from behaviorism, and which enables him to treat his facts from a conditional-genetic standpoint instead of recounting them as so many examples of phenotypes. These tensions moreover cannot be dismissed by the behaviorists as mere inventions of Lewin's, introduced by him to support his theories, to be regarded, if not exactly in the same category as their own concepts of visceral tension, at

¹¹ By this must not be understood phenomenological field. The two are not even always parallel. The strength of a psychic tension by no means corresponds with the strength of phenomenological desire. Many tensions are indeed unconscious. I will in passing pick up a book from the floor or straighten a cover without even realizing I am doing so. Lewin is no mere introspectionist.

¹² Another word of warning is perhaps here necessary lest we should imagine that Lewin in making use of such concepts as force, system, tension, is necessarily referring these back to physics. Such a question he leaves open. He points out that such dynamic concepts are valid outside physics and are being used today in other sciences, such as the science of economics. He maintains indeed that they are rather to be considered as the basic concepts of a new logical system—the Logic of Dynamics.

any rate as parallel suppositions, equally remote from actual proof. Lewin is able, in direct experiment both to show the existence of psychic tensions and to measure their intensity.¹³ This is his great advance. He has opened up a new experimental field, amenable to direct and tangible investigation and proof. Along the lines of memory, habituation, volition—almost all paths of activity—he has started investigations which substantiate his theories and besides disclosing further facts with regard to their own functioning, suggest new problems to explore. He no longer merely describes, he experiments.

In summarizing Lewin's first paper we see that as causal factors governing behavior we must find sources of energy which cannot be mere couplings nor the stimulus alone. These energies are to be found in psychic tensions which tend to discharge so that the psychical field returns to equilibrium. Their influence, in a causal-dynamic sense, extends into phenomenological, physical and physiological fields, without however their belonging directly to any one of them. The energy due to the tension is the actual cause of the behavior which is governed in its course by the forces in the outer field as well as the inner.

In Lewin's second paper (2) we find further theoretical considerations and the first experimental results. He begins by attacking the problem of volition. Here we have hitherto had few satisfactory investigations because under the term Will has been placed a very heterogeneous group of phenomena. Lewin drops all inquiry into the phenomenology of willing and proceeds to study dynamic causes. The process of intention or purpose is usually considered to fall into a motivating process, an imagined solution, and the carrying out of the imagined solution, due to its association with the real opportunity of carrying it out.¹⁴ Such was the association theory of voluntarily controlled behavior. The actual motor for carrying out any act—let us say, posting a letter—

¹³ Cf. Zeigarnik's experiments, in which the strength of the tension is measured by its influence upon memory in the retention of acts performed.

¹⁴ For a review of the literature on the Will, Purpose, etc. see Lindworsky, 'Der Wille.'

was due to the association of a real mail-box with the posting rehearsed in the imagination. That such a coupling cannot be a causal factor Lewin has shown both theoretically and experimentally in the problem of measuring will against association.¹⁵ Furthermore, the association theory will not stand in face of everyday obvious facts. In the first place, after the act is fulfilled, we find no tendency to attempt another fulfillment when confronted with a new opportunity. The association hypothesis would imply a strengthening of the association and hence a tendency to repetition. Thus, I have a letter to mail and decide to mail it when I pass the post-box on my way to the office. I do so and pass the same post-box ten times again that day without even thinking of the mailed letter. Secondly, the act of fulfillment may vary decidedly from the original imagined one; a friend may drop into my room and I give him the letter to mail. Once again I find no tendency to mail the letter as I pass the box. One more criticism of the many that Lewin advances may be mentioned. I decide to give the letter to my friend whom I am expecting. He fails to keep his appointment and on my way to the office I mail the letter myself. Here we have a simple case of 'preconceived act' (*Vornahme*) which may or may not be executed in the intended manner. Lewin believes that a psychic tension is set up upon the occasion of writing the letter, which like all tensions is a force in the direction of discharge. The line of discharge will be governed by internal and external field forces.

Thus Lewin has developed a dynamic psychological concept which we may take as the starting-point of experiment. Moreover we are attacking the problem from the conditional-genetic side, instead of, with the behaviorists and reflex-ologists, becoming confused on the study of phenotypes. The experimental work to prove the existence of this tension and to measure it, we will deal with later. (*Cf.* the experiments of Ovsiankina which show that when an act is interrupted the tension behind it remains to cause its resumption.)

¹⁵ See Lewin's papers on this problem (6), (7). To be sure more modern investigators have supposed 'determining tendencies' and other dynamic factors to help out association, but as Lewin points out, the critique can be carried on to them with little difficulty.

Forgetfulness in the sense of failure to carry out an intended act, due to what is popularly thought of as absent-mindedness, can also be more adequately explained by Lewin than by his predecessors. As a usual thing the intended act is not forgotten when the system in which it is embedded is active. Forgetting may be due to a compensatory fulfillment of the intended act. For instance, I make a note of something I wish particularly to remember and forthwith dismiss it completely from my mind. Here we have a compensatory fulfillment. The 'writing it down' has compensated for the actual fulfillment. 'Forgetting' in the Freudian sense (*Vergessen*) is accounted for in that the intended act comes into direct collision with tensions built up on 'genuine needs' (*echte Bedürfnisse*). The tensions built up upon these 'needs' are dynamic structures which are open to measurement and investigation.

I leave unmentioned other theoretical implications to go on to Lewin's concepts of genuine (*echte*) and derived-needs (*quasi-Bedürfnisse*) which are the sources of energy in volitional behavior. Our reactions, contrary to some sensationalistic viewpoints, are to real things and situations. Certain things in our environment have or acquire the stimulating character (*Aufforderungscharakter*) already mentioned. This stimulating character varies and may be negative as well as positive. It is, as we have pointed out, nothing inherent in the stimulus itself, but is due to the relationship between tensions and the forces in the field of behavior. The mirror which constantly stimulates me positively to go and admire myself in it loses this character absolutely when I require it as a piece of apparatus. I do not so much as glance in it. Through satiety (*Sättigung der Bedürfnisse*) the positively stimulating character of a situation may become negative. From the standpoint of energetics we have a complete discharge of the tension, with a new tension built up in the opposite direction. Thus the need and the stimulating character are different aspects of the same general dynamic concept. We can measure the amount of the need by the stimulating character of the stimulus with which it is connected.

The 'genuine needs' are due to upsets in the equilibrium of the organism and are manifested by tensions which exert real forces. These are something more than the drives of other dynamic psychologies. They may be measured on actual physiological change in the organism concerned, but in many cases we can show their existence psychologically and deal with them experimentally, while physiological changes are, with our available instruments, quite immeasurable. The needs may be of an outright physiological nature such as hunger or sex, or they may be socially conditioned such as the need to follow a certain career, the need of the artist for a certain type of self-expression.

The tension back of a preconceived act is termed a derived need by Lewin. This is not to be understood in the old sense of a mere coupling between the stimulating character of a situation and the need. Factually the derived need represents a tension as real as that of a genuine need. The execution of the preconceived act leads to a discharge of this tension and the disappearance of the derived need. When the tension is too great, it may break out before the actual intended situation presents itself. In the same way a genuine need may suddenly discharge in overt behavior. The stimulating value of a situation is, as we have seen, dependent on the need and increases in proportion to the same. A starving man will eat with avidity things that would repel him in his normal condition. Fixation is important. The stimulating character of the situation in which the first discharge of a function occurs, *i.e.*, the first satisfaction of a need, tends to become strangely positive. This is well known in genuine needs (as for instance in sex) but it can also be shown to occur in the fulfillment of preconceived acts. The tension then discharges automatically under certain situations. The jump from here to habit is not a great one.

Compensatory fulfillment (*Ersatz-Erledigung*) plays an important rôle. Lewin distinguishes between three types of compensation. In the first case fulfillment is brought about by some difference arising in the intended situation. I meet my friend and give the letter to him instead of mailing it

personally. Or there may be a *pars-pro-toto* compensation. I go upstairs to fetch something and forget to bring it down with me. Thirdly, we have substituted compensation (*Surrogat-Erledigung*). Jilted by his best girl, the disappointed lover is all too often 'caught on the rebound.' Or the baby cries for the moon and is content with a penny whistle. This type of compensation is closely related to the symbolism of Freud. Of the possibility of testing Freudian mechanism experimentally, we shall speak later. (Cf. Dembo's experiments.)

There is furthermore an essential relationship between the genuine and the derived need. The derived need as manifested by the preconceived act is always built up on some genuine need. I write my friend a letter out of an actual desire for his company. When the intended act stands in direct opposition to some genuine need it can never be fulfilled. This is particularly illuminating in certain facts about hypnosis. Thus we cannot instruct a subject to commit murder. That the derived need is built up on a genuine need, we shall see later. Indeed the difference between the two is, in the last resort, only a difference in degree. The derived need must have its foundation in the genuine need.

With such evidence as he has here gathered together, Lewin sees the importance of reconsidering our volitional concepts. In the first place the rôle of the 'preconceived' act is a much smaller one than is generally supposed. How often in real life do we intend—that is, with the accompanying imaginative processes—to do things before we actually do them. Such terms as will and purpose Lewin discards and draws a distinction instead between controlled and uncontrolled behavior (*beherrschte und unbeherrschte Handlungen*). 'Uncontrolled' behavior is guided entirely by the forces in the field in which it occurs—thus including internal psychical as well as external physical forces. Thus an outburst of rage is due to inner psychic forces but is an example of uncontrolled behavior. 'Controlled' acts are also subject to field forces, but here the organism as a coherent system

has the upper hand. 'Volitional behavior' can thus be placed in neither category exclusively. It may be controlled or uncontrolled; a child makes up its mind to go past a dog but scuttles by in an uncontrolled and purely instinctive manner. The driver on the other hand who by a skillful swerve avoids sudden catastrophe exhibits controlled behavior without any preceding 'intention.'

Here we have Lewin's own description of the essence of intention. "In an intention the individual so transforms the outer and inner field for a future moment, that he will then, even though his behavior may be uncontrolled, accomplish what he has now in mind. Preconceiving only occurs when the individual foresees that a situation will arise where he would otherwise instinctively behave differently." The child resolves not to take more than two pieces of cake at the party. One's daily tasks are performed without any such preconceived intentions.

Thus we can account for every possible variation in the method of execution of an intended act, which as we have seen the association theory cannot do. In a separate category belongs absolute decision (*Entschluss*). This occurs when the entrance to the motor component for a tension is made possible in a way that did not exist before. The *fiat* of James accompanies this, clearing the path.

The intended act and the derived need are not isolated mental phenomena but are embedded in different complexes and stand in communication with other derived and genuine needs. A tension due to a genuine need discharges when the need and a definite situation come together. Thus the actual type and execution of intended acts depends largely on field forces.

We have now covered the essential points of Lewin's program papers. We find developed dynamic concepts such as tension and field that are conditional-genetic at the same time. Essentially Lewin views behavior as tensions discharging in definite fields. It now remains for him to give us his actual evidence for the existence of these tensions and his manner of measuring them.¹⁰ We come then to the

¹⁰ The other side of the question—How do the tensions in the organism actually arise?—Lewin admittedly leaves for the present unanswered.

experimental work on these problems, and take up Zeigarnik's paper 'on the retention of completed and incomplete acts' (3).

We have seen that it is not necessary for the imagined opportunity to present itself before the preconceived act shows a tendency in the direction of discharge. Instead of that, the intended act is characterized by the derived need and its dynamically equivalent tension. This tension will make itself felt in every possible direction, and the purpose of Zeigarnik's experiments is to show its presence when an act has not been completed and to show the effect of such a tension upon memory performances related to the act.

She sets out to measure the difference in the retention of interrupted and completed acts. The tension in the first place remains, in the second it is discharged through the fulfillment.

The experiments were performed on 164 subjects, children, students, teachers; and two group experiments, over 40 to the group, were held.

The procedure was to give the subject twenty-two simple tasks to perform and allow him to finish one-half of these, to interrupt him in the other half. Immediately afterwards he was asked to list them. The order was unessential to the listing. Needless to say the acts were interrupted in chance order, different ones interrupted in different experiments and the usual repetition controls used.

I give a few of the acts called for:

To write down a poem from memory.

To draw one's monogram.

To draw a plan of a particular section of Berlin.

To string beads.

To count backwards from 55.

To solve simple match puzzles.

To find two triangles (out of thirty all different) which fitted together to form a square.

The subject's introspection was taken after the list was made. Thus there was a control over the subject's insight into the purpose of the experiment, his attitude towards

it, the degree of indifference, etc. The results of the experiments are given in terms of the quotient RI/RC that is $\frac{\text{retained interrupted}}{\text{retained completed}}$. The first experiment showed that the arithmetic mean of $RI/RC = 1.9$, *i.e.* the interrupted tasks were retained much better (90 per cent) than the completed acts; the single subjects showed quotients varying from 6 to .75. The results of the individuals showed that in 17 cases $RI/RC > 1$, in two cases $= 1$, and in only three cases < 1 . A repetition with different subjects and different tasks gave practically identical results. Also the group experiments showed for adults a.m. of $RI/RC = 1.9$, for children a.m. of $RI/RC = 2.1$.

The introspection showed that very rarely did a subject see the point of the experiments. They were usually assumed to be intelligence tests or something entirely different from what they really were. The motivation was usually discovered to be (1) ambition to excel, (2) feeling of duty towards experimenter, (3) interest in the task itself.

It is interesting to note that subjects often objected to the interruption and attempted to resume the interrupted acts.

To show that the retention is not due to the shock of interruption, experiments were performed where half of the experiments were interrupted and later allowed to be completed, the other half remaining unfinished.

$$\begin{array}{l} \text{In this case the quotient } \frac{R(I + C)}{RI} \text{ i.e.} \\ \frac{(\text{Retained interrupted and later completed})}{(\text{Retained interrupted})} = 1.9. \end{array}$$

The same results, we see. A further experiment with the same subjects in which the two methods were combined correlates for the individual subjects $\rho = .90$ (Spearman rank difference method).

To meet the explanation that the interrupted are better retained than the completed because the subject might expect to finish them later, experiments were conducted in which the subject was told at each interruption 'we'll finish

that later,' with such emphasis on the finishing later that we should expect RI/RC to rise. It becomes, however, lower than formerly, 1.7.

All other possibilities are considered and controlled, and we must find the cause of the retention in the tension set up by the derived need behind the intended act. These tensions persist and become evident in the better retention of RI .

The results so far are not astounding. It is to be noticed, however, that RI for all cases stands at about 7. The variation in RI/RC is localized in RC . Introspection showed that certain subjects viewed the listing as a memory test, others as merely the final task of a series. When these groups are separated, RI/RC for those that viewed the listing as merely another task, rises to 3.8! Thus, besides the tension due to the quasi-need we have others in force. These others work in different degrees and further experimental analysis exhibits them. In cases where the subject is not satisfied with the completion a very high percentage is returned, those interested in the experiments show an increase in RC while RI remains constant. Other controls and analyses we do not mention through lack of space.

Zeigarnik comes to the following conclusions. There is an actual tension present due to the derived need, that works towards the completion of an act begun and also affects the retention of this act. The amount of this retention is dependent on the intensity. When the subject's ambition is aroused, that is, when a genuine need is raised, RI/RC grows enormously. Acts which naturally have a definite completion point, arouse a much stronger tension than those which have no such definite end position. Tensions are not set up unless there is a certain stability in the whole psychic field. In cases of fatigue and excitability no tensions are set up. Subsequent emotional upsets can cause existing tensions to explode (thus making $RI = RC$). After a certain lapse of time, *i.e.*, in the course of a few days, the tension also disappears from natural causes, again making $RI = RC$. Children show a greater tendency to develop tensions than adults. Lewin through Zeigarnik's work has been able to

prove the existence of psychic tensions and measure them in different situations.

The second paper of the series, by Schwarz, deals with 'Regressions in Rehabilitation.' The former studies of regression, among others those of Münsterberg, have all been from the standpoint of quantitative performances, such questions as the possibility of rehabilitation, the time necessary, etc., being those considered. Lewin is interested from the conditional genetic side. What are the tensions operating, how do they operate, what is the nature of the process of rehabilitation?

The experiments set up by Schwarz require the subject to go through a series of acts, which form a connected whole (*Handlungsganzheit*). One of these acts in the series is varied in its method of execution. It is in this act that we find regressions (reversions to an habitual type of behavior). The apparatus used was very simple. A marble is placed in a trough and drops into a closed box. It is then ejected from the box by pressing a lever, caught in the hand, and placed in a tray. The variation was in the lever component, which could be set by the experimenter so that instead of pressure, lifting was required.

To summarize briefly the results of these experiments. When one develops in the subject the habit of pressing the lever in carrying out the whole act and then changes the arrangement so that the lever must be lifted, a certain number of performance errors result. These fall under two heads, errors due to regression where there is an actual drive towards carrying out the habitual act, and errors due to confusion where there is no drive but merely doubt in the subject's mind as to which to do. In both cases these errors are graded from actually executed errors to a mere tendency towards error.

That either type of error occur, the change must be made in a part of a total act, that is, in one part of an act that can be viewed as an integrated whole. In other cases there is no tendency towards regression or confusion errors. The part changed cannot be the chief act or there is also no regression.

In other words errors only occur when a dependent part of a Gestalt act is changed.

We have to distinguish between habits based on instinct or drive, as in the case of the drug fiend, and mechanical habits of action, such as we acquire, for instance, in the way we switch on or off the electric light. The case in hand is an example of the latter. The energy responsible for the execution of the part action, whether it is the correct re-habituating or the regression, is obtained from the derived need which is responsible for the total act. There is no independent source of energy. The reason why mistakes occur is that this part action has become embedded as an absolutely dependent part in the Gestalt of the total action; there is no other source of energy present to correct this until it is finally overcome by an increasing amount of self-control, which enables the parts of the total-action to become more directly dependent on the derived need and less embedded in the Gestalt of the total act.

When we change the act back to the old original habit act, *i.e.*, in our example pressing instead of lifting, the reversion errors become decidedly less. Theoretically we are dealing now with a two-track structure. There are two possible motor tracks over which the tension may discharge; after changing the original act to the re-habituating act several times, the tendency to regression is zero.

At this level there are no longer errors of regression, but confusion errors often manifest themselves. This occurs when the two-track motor system is thoroughly built up. If one greatly increases the positively stimulating character of the lever either as pusher or lifter, the action right from the beginning proceeds smoothly and without error.

When we summarize the results, we find that we are not dealing psychologically with a mere summary of movements, $a + b + c + d + e$, but that the total integrated act is of a Gestalt nature. When we have developed habitual response of the pushing type, we can represent this as follows, $a(b.c.d.)e$. *A* represents choosing a marble, *b.c.d.* throwing it in the trough, pushing the lever, catching it, and *e*, placing it in the tray.

When we change c (pushing) to c_2 , lifting, the general Gestalt nature is changed. Hitherto a and e were relatively independent part actions as against the relatively interdependent group of the three actions $b.c.d.$ To enable c to be successfully replaced by c_2 , this Gestalt formation must be broken up and c becomes emphasized, so that the total act now looks like this $(ab) c_2 (de)$; in this way the other variant c is kept suppressed. Additional experiments not yet published serve to bring out further the Gestalt nature by showing that no regression errors occur when all the parts of the middle act are changed and when it is no longer a case of a difficult transformation (*Umgestaltung*), or isolating a part act that has become deeply embedded in another configuration. Lewin, we see, is thus able to attack the problem of re-habitation from the conditional-genetic side and we gain some insight into its nature.

Another paper, by Ovsiankina 'On the resumption of interrupted acts' is now in the process of publication. It deals with further manifestations of the derived needs and tensions in experiments similar to those of Zeigarnik. Whenever an act is begun, a tension is set up. What happens to this tension when the act is interrupted? That it remains to influence memory is shown by the Zeigarnik experiments. Ovsiankina shows that when an act is interrupted there remains in the subject a natural tendency towards resumption. Tasks similar to those in the Zeigarnik experiment were set. The interruptions were either 'disturbance' interruptions such as deliberate requests from the experimenter to start something new or 'chance' interruptions. There were several kinds of chance interruptions. The lights went off automatically, the experimenter dropped a drawer and requested the subject to help pick up the contents, etc. The subjects were occasionally interrupted by being asked to give introspections; several other such devices were also made use of. The tasks set were also divided into end acts, or those with a definite finished point, and continuous acts, those upon which an indefinite amount of time could be spent. A puzzle belongs to the first type, while drawing lines at a certain

distance from each other on a piece of paper is an example of the second. The procedure was simply to note whether or not the subject resumed or showed any tendency to resume the interrupted act. Only twenty seconds were allowed for the resumption.

The first experiment showed that of the C.I. (chance interruptions) 100 per cent were resumed. Of the D.I. (disturbance interruptions) 91 per cent were resumed. Needless to say only about half of the tasks were interrupted.

That the resumption was not due to mere boredom with the situation, was shown by the insertion of a pause for rest after completed acts. No desire for mere activity was exhibited. When the experimenter expressly forbade resumption, resumptions of an underhand nature occurred. That is, the subject took up the task again in a sly manner when the experimenter was not looking. That it was not mere interest which caused the activity, was shown by the introspections of the subject. The acts in themselves (*cf.* list in Zeigarnik) are not of the type to arouse interest in adults. In addition, acts positively unpleasant to the subject were often undertaken.

Experiments were also done on the influence of the time of interruption. It was found that tasks interrupted towards the beginning were invariably resumed. This 100 per cent falls occasionally to 60 per cent around the middle of the task, but rises again to 80 per cent in the end spurt. Experiments also yielded examples of compensatory and crude fulfillment.

Actual fulfillment, as we have seen in the Zeigarnik experiments, discharges the tension. Ovsiankina found there was no tendency to resume a task which had once been completed. Acts which normally have no interest for the subject acquire a positively stimulating character when the derived need and tension towards completion is once set up. Thus we have additional proof of the validity of Lewin's concept of psychic tension.

From the other investigations in this series which have not been published I choose a few that give clear results.

Birnbaum finds that 'forgetting' in the sense of failing to carry out an intended act can be experimentally shown at times to be due to compensatory fulfillment. Thus in a series of tasks the subjects were instructed after completion of each task to write their names on the papers. The task of monogram writing served usually as compensation and with this task the name was forgotten with astounding regularity. Other examples of marked compensatory fulfillment have been found by Dembo. The subject was required to throw rings over a bottle (an almost impossible task) and a certain amount of satisfaction and hence discharge was found to occur when these were thrown over hooks or near-by articles. In instructing the subject to obtain certain flowers that could not be reached, it was found that others which could, but which from the standpoint of the experimenter were worthless, served to discharge tension.

Lewin's general manner of attacking a problem is worthy of a few words. The experimental situations chosen, as we have seen, make use of total acts. Rather than validity through repetition Lewin stresses validity through careful control and variation. Careful records of the course of each experiment are made. After each experiment a complete introspection is taken. The interpretation is always, however, from the descriptive protocol. The introspections are only treated as decisive when, all other factors being equal, the results are capable of a two-fold interpretation. Particularly useful is Lewin's use of the film to secure as thorough a record as possible. Whenever practical he secures a cinematographic record of the course of behavior.¹⁷

Let us now sum up what Lewin has actually accomplished. At this point the American psychologist may at first be inclined to be dubious. Surely, he will say, the outcome of such experiments as those of Zeigarnik, Schwarz and Ovsiankina are statistical regularities of no very marked predictive value. Where are the absolute psychological laws of which Lewin has made so much? Such a criticism would

¹⁷ For Lewin's use of film see particularly 'Filmaufnahmen' etc. (9) and 'Kindlicher Ausdruck' (10).

mean a complete misunderstanding of Lewin's attitude. He is not yet able to set up his laws, but is simply in a position to show the material from which they must eventually come. That is, he has shown that any law must be a genotypic description of behavior, that the associationists and behaviorists have confused the genotypic and the phenotypic. He is able logically to prove the existence of tensions, to measure them roughly and indicate that dynamic laws must be in terms of energy exchanges and field equations. His most important contribution is methodological rather than factual.

His factual contribution, however, should not be underestimated. The investigations are also pedagogically interesting. Certain results tend to corroborate and shed light on the Freudian and Adlerian mechanisms. These concepts, behind which a great deal of value undoubtedly lies, have hitherto received no adequate experimental criticism from the laboratory psychologists. Roughly speaking, Freud, too, is dealing in genotypes. Thus we can see compensation experimentally investigated by Dembo. From Zeigarnik's experiments we see certain corroboration of the Freudian doctrine of memory. In pointing out the possibility of laboratory experimentation on psychoanalytic mechanisms, Lewin has done something that will tend to clarify our heterogeneous science. That he is not yet able to measure with great exactness lies in the newness of real dynamic concepts for psychology. One remembers that the first electric potentials were measured on the legs of a frog, that the clock was not possible as a time-piece before the days of Galileo. Lewin is certainly able to set up, measure and predict psychic energies with as much accuracy as the physicist used in the early days of dynamic concepts in his science. Like all pioneers, his work, rather than to dictate finished laws, has been to indicate directions and open up new paths of experiment from which the laws must eventually come.

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THEORY OF ATTITUDE MEASUREMENT

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It is the purpose of this paper to describe a new psychophysical method for measuring the psychological or functional similarity of attributes. Its development was motivated primarily for the solution of a particular problem in the measurement of social attitudes and it is in terms of this problem that the new psychophysical method will be described.

Let each of a group of N individuals be labeled as to the presence or absence of each of n attributes. This means that we are dealing with N persons and that each of these persons declares the presence or absence in him of each of the n attributes. It does not matter for our present purposes whether the declarations are made by these people for themselves or by others for them. In our particular problem we are dealing with a list of n statements of opinion and each person has the option of endorsing or rejecting each of the n opinions. The statement of an opinion is here regarded as a description of an attribute and the subject merely indicates whether he possesses the attribute. A similar analysis might be made for a series of traits which are supposed to describe people along an extroversion-introversion continuum, an ascendance-submission continuum, and so on. Our primary interest is here in the attitude continuum.

We postulate, for verification, an attitude continuum for the n opinions. Let them describe different attitudes toward the church for purposes of illustration. Some of the opinions reflect attitudes very favorable and loyal to the church; others are neutral or slightly favorable, while still others are slightly or strongly antagonistic to the church. We want an objective procedure for ascertaining whether any particular set of opinions really behaves as a continuum when the endorsements are analyzed.

Let us consider first a pair of opinions, one of which is clearly favorable to the church and the other as clearly antagonistic.

1. I feel the church services give me inspiration and help to live up to my best during the following week.
2. I think the church seeks to impose a lot of worn-out dogmas and medieval superstitions.

Now, on a common sense basis, we should expect to find that of the people who endorse opinion 1 relatively few will endorse opinion 2. Similarly, those who endorse 2 will only seldom endorse 1. The following pair of opinions would probably behave differently.

1. I feel the church services give me inspiration and help to live up to my best during the following week.
3. I believe the church is the greatest influence for good government and right living.

If we consider the group of people who endorse 1 we should expect a rather high proportion also endorsing 3 because the two statements are both favorable to the church. The attitudes represented by these two statements may be expected to co-exist in the same person while 1 and 2 are more or less mutually exclusive.

These facts suggest the possibility of measuring the psychological similarity of opinions in terms of the endorsements. For the two opinions we shall have the three following facts:

- $n_{(1)}$ = the total number of individuals in the group N who endorse opinion No. 1.
- $n_{(2)}$ = the total number of individuals in the group N who endorse opinion No. 2.
- $n_{(12)}$ = the total number of individuals in the group N who endorse both 1 and 2.

Other things being equal, a relatively high value for $n_{(12)}$ means that the two statements are similar. A relatively low value for $n_{(12)}$ means that the two opinions are more or less mutually exclusive.

We shall avoid mere correlational procedures since it is possible in this case to do better than merely to correlate the

attributes. When a problem is so involved that no rational formulation is available, then some quantification is still possible by the calculation of coefficients of correlation or contingency and the like. But such statistical procedures constitute an acknowledgment of failure to rationalize the problem and to establish the functions that underlie the data. We want to measure the separation between the two opinions on the attitude continuum and we want to test the validity of the assumed continuum by means of its internal consistency. This can not be done if we had merely a set of correlational coefficients unless we could also know the functional relation between the correlation coefficient and the attitude separation which it signifies. Such a function requires the rationalization of the problem and this might as well be done, if possible, directly without using the correlational coefficients as intermediaries.

Before summarizing these endorsement counts into an index of similarity we shall introduce another attribute of the statement, namely, its reliability. Suppose that there are $N_{(1)}$ individuals in the experimental population whose attitudes toward the church are such that they really should endorse statement 1 if they were conscientious and accurate and if the statement of opinion were a perfect statement of the attitude that it is intended to reflect. Now suppose that as a result of imperfections, obscurities, or irrelevancies in the statement, and inaccuracy or carelessness of the subjects, there are only $n_{(1)}$ endorsements of this statement. Then the reliability of the opinion would be defined by the ratio

$$p_1 = \frac{n_{(1)}}{N_{(1)}}. \quad (1)$$

The notation p_1 means the probability that the statement will be endorsed by a subject who would endorse it if he were accurate and if the statement were a perfect expression of the attitude it is intended to convey. The question naturally arises as to how to ascertain the value of N_1 which could be obtained directly only if the statement were perfect and the subjects absolutely accurate.

We shall consider three methods of determining approximately the reliability of each statement.

1. Let the whole list of opinions be presented twice in random order. If there are fifty opinions in the experimental list there would be one hundred opinions to be read to the subjects, each opinion being repeated once. Let the endorsement counts for opinion 1 be as follows.

n_1 = total number of subjects who endorse the first presentation of No. 1.

n_1' = total number of subjects who endorse the second presentation of No. 1.

n_{12} = total number of subjects who endorse both presentations of No. 1.

The proportion of those who checked 1 who also checked 2 is

$$p_1 = \frac{n_{12}}{n_1} \quad (2)$$

and we shall assume that this proportion is the same as the proportion of those whose attitudes are of opinion 1 who actually checked that opinion. In other words,

$$p_1 = \frac{n_1}{N_1} \quad (1)$$

Similarly for the second presentation of the same opinion we have

$$p_1 = \frac{n_{12}}{n_1'} = \frac{n_1'}{N_1} \quad (3)$$

But we can not expect the experimental values of n_1 and n_1' to be exactly the same so we shall use them both for determining the value of p_1 by the product of (2) and (3) so that

$$p_1^2 = \frac{n_{12}^2}{n_1 n_1'}$$

and hence

$$p_1 = \frac{n_{12}}{\sqrt{n_1 n_1'}} \quad (\text{reliability of an opinion}). \quad (4)$$

2. A second procedure which should give at least roughly

comparable results is as follows. Let the entire list of opinions be sorted into any convenient number of groups by the method of equal appearing intervals. The statements may then be placed in rank order from those that are most antagonistic to the church to those that are most favorable. The detailed procedure for this scaling has been described elsewhere.¹ Then any two adjacent opinions will reflect practically the same attitude especially if the list is as long as 40 or 50 opinions or more over the whole available range of the attitude continuum.

Let any two adjacent opinions in this rank order series be numbered 1 and 2 respectively. The total number of individuals in the experimental population whose attitudes are represented approximately by the adjacent opinions 1 and 2 may be designated N_{12} . If both of the statements were perfect and if the subjects were absolutely accurate, then we should expect to find n_{12} to be very nearly equal to N_{12} , which is the full number of subjects whose attitudes are that of opinions 1 and 2. Strictly speaking, we are combining here two factors of reliability into one, namely the reliability of each opinion and the mean conscientiousness of the subjects. The reliability of the statement is the probability that a subject will endorse it if the subject's attitude is that of the opinion. It is a function of such characteristics of the statement as obscurity, subtlety or indirectness of its meaning, or actual ambiguity in its meaning. The reliability of the subject is the probability that he will endorse the opinions that he really should endorse in order truly to represent his attitude. This reliability is a function of such factors as the conscientiousness of the subject and the experimental arrangement. If the subject is asked to read several hundred statements of opinion he will not read them so carefully as if he is asked to read only a dozen. But we have combined these factors of reliability into a single index, the probability that the statement will be endorsed by the people who should

¹ For distinction here made between opinion and attitude see Thurstone, L. L., 'Attitudes Can Be Measured,' *Amer. J. Sociol.*, 1928, 33, 529-554. In this paper is described the construction of an attitude scale by the method of equal appearing intervals.

endorse it. If this type of analysis should prove to be fruitful there will no doubt be further investigations in which these factors of reliability are analyzed separately and explicitly.

Since there are N_{12} individuals who should check opinion 1 and since the actual number who checked this opinion is only n_1 the probability that this statement will be endorsed by those who should endorse it is

$$p_1 = \frac{n_1}{N_{12}} \quad (5)$$

and, similarly,

$$p_2 = \frac{n_2}{N_{12}} \quad (6)$$

These two probabilities are assumed to be practically uncorrelated so that

$$n_{12} = N_{12}p_1p_2 = \frac{n_1n_2}{N_{12}} \quad (7)$$

or

$$N_{12} = \frac{n_1n_2}{n_{12}}, \quad (8)$$

and hence

$$p_1 = \frac{n_{12}}{n_2} \quad (9)$$

and

$$p_2 = \frac{n_{12}}{n_1} \quad (10)$$

The assumption that the two probabilities of endorsement are uncorrelated is probably incorrect because the subject who is conscientious in reading one of these opinions will of course be likely to be conscientious also in reading the second opinion and consequently the probability that the two opinions will both be endorsed is not, strictly speaking, the product of the two separate reliabilities. The approximation is perhaps sufficient for our purposes and it may be hoped that it introduces no violent error.

The above procedure enables us to estimate the reliabilities of the opinions in terms of known data but this particular method requires that the opinions in the experimental list be first sorted into a rank order series by the method of equal appearing intervals or into a simple rank order series.

3. A third procedure is really identical with the second above except that instead of obtaining adjacent opinions by submitting the entire series of opinions to a large group for sorting, the experimenter selects adjacent statements by inspection. This is certainly not a safe procedure and it should be discouraged. A modification that could be acceptable is to select pairs of opinions that are paraphrased forms of the same statement, and then apply the logic of the second procedure above. It is by no means certain that these three methods of determining the reliability of a statement will give similar values. It might very well happen that the first procedure described above gives a measure of reliability in terms of factors more restricted than those which enter into the second and third procedures. If such is the case the first procedure gives values that are too high while the second and third procedures may give values more appropriate to our purposes.

We now have the following statistical facts about the two opinions whose separation on the attitude continuum is to be ascertained.

n_1 = total number of individuals who endorsed opinion No. 1.

n_2 = total number of individuals who endorsed opinion No. 2.

n_{12} = total number of individuals who endorsed both opinions.

p_1 = reliability of opinion No. 1.

p_2 = reliability of opinion No. 2.

Let one of the opinions have its scale value at S_1 on the attitude continuum of Fig. 1. Let there be N_1 persons in the experimental group who should endorse it if they were absolutely accurate and if the statement of opinion were a perfect representation of the attitude it is intended to convey. The actual number of subjects who really do check this opinion is

$$n_1 = N_1 p_1, \quad (II)$$

in which p_1 is the reliability of the statement.

Now consider another statement whose scale value is at S_2 on the attitude continuum. Since there is a difference $(S_2 - S_1)$ between the attitudes of these two statements we should not expect all of the n_1 subjects to endorse this second statement. If it were perfect in reliability, then the number of subjects in the group n_1 who also endorse statement 2 will be

$$n_1\phi = N_1p_1\phi, \quad (12)$$

where ϕ is some value less than unity. Now, it is reasonable to assume that if the two statements are far apart on the scale, then the proportion ϕ of the group n_1 who also endorse the distant statement 2 will be small. This is represented in

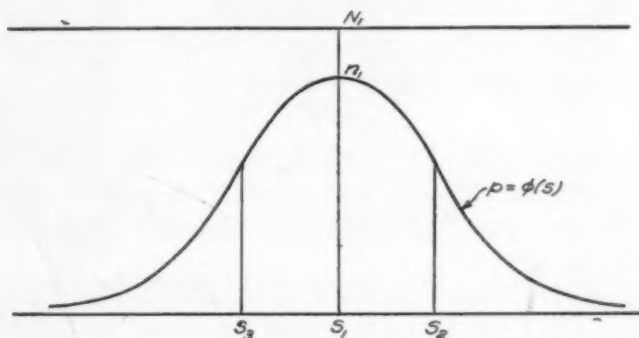


FIG. 1.

Fig. 1 by the fact that ϕ is a maximum when the separation $(S_2 - S_1)$ is small while it approaches zero as this separation becomes large. We shall assume that this function is symmetrical about the axis at S_1 so that

$$p_{1(k)} = \phi(S_k - S_1)^2, \quad (13)$$

in which $p_{1(k)}$ is the proportion of those who endorse statement 1 who also endorse statement k while S_k is the scale value of statement k . Our assumption is that $p_{1(k)}$ is a function of the separation $(S_k - S_1)$ but that it is independent of the sign of the separation which is an arbitrary matter.

We must also take into consideration the fact that statement 2 is probably imperfect as well as statement 1. Let its

reliability be p_2 and we shall then say that the number of those who check statement 1 who also check statement 2 is

$$n_{12} = n_1\phi p_2 = N_1p_1p_2\phi. \quad (14)$$

The number of those who checked No. 1 who also checked No. 2 is of course the same as the number of those who checked No. 2 and who also checked No. 1. Hence, we may write, by analogy,

$$n_{12} = N_2p_1p_2\phi \quad (15)$$

or

$$n_{12} = n_1p_2\phi, \quad (16)$$

$$n_{12} = n_2p_1\phi,$$

and hence

$$n_{12}^2 = p_1p_2n_1n_2\phi^2 \quad (17)$$

or

$$\phi = \frac{n_{12}}{\sqrt{p_1p_2n_1n_2}}. \quad (18)$$

This is the coefficient of similarity of two statements of opinion. When this value is relatively high, the two statements belong close together on the scale but when ϕ is small they are far apart.

This formula may be used to determine the reliabilities p_1 and p_2 if the two statements are known to have practically the same scale value. This fact may be known either because of the fact that they are statements of the same idea, one being a paraphrase of the other, or by being scaled by the method of equal appearing intervals, previously described. Since the coefficient ϕ deviates from unity supposedly only on account of the scale separations, it is in this case unity, the two statements having practically the same scale value. Then

$$1 = \frac{n_{12}}{\sqrt{p_1p_2n_1n_2}}. \quad (19)$$

But

$$N_1 = N_2,$$

since both of these symbols represent the same quantity, namely the number of people in the total group who should endorse both statements at the same scale value if both

statements were perfect. Hence

$$N_1 = N_2 = \frac{n_1}{p_1} = \frac{n_2}{p_2} \quad (20)$$

or

$$n_1 p_2 = p_1 n_2 \quad (21)$$

and

$$p_2 = \frac{p_1 n_2}{n_1}. \quad (22)$$

Substituting (22) in (19) we have

$$I = \frac{n_{12}}{\sqrt{p_1 p_2 n_1 n_2}} = \frac{n_{12}}{p_1 n_2}, \quad (23)$$

and hence

$$p_1 = \frac{n_{12}}{n_2} \quad (\text{for identical or adjacent opinions}), \quad (24)$$

and, by analogy,

$$p_2 = \frac{n_{12}}{n_1} \quad (\text{for identical or adjacent opinions}). \quad (25)$$

Our next problem concerns the exact formulation of the function

$$\phi_{1k} = f(S_k - S_1).$$

We shall try first the assumption that it is Gaussian so that

$$\phi_{12} = \frac{1}{\sqrt{2\pi} \cdot \sigma} e^{-\frac{(s_1 - s_2)^2}{2\sigma^2}}. \quad (26)$$

This means that if a man has endorsed any particular perfect statement No. 1, the probability that he will also endorse another perfect statement No. 2, distant from No. 1 by the scale separation $|S_1 - S_2|$, is assumed to be a Gaussian function of the scale separation. This assumption can be tested empirically by the internal consistency of the scale, but the function can also be studied directly without this assumption by methods that will be left for separate publication. We shall assume for the present experiment that this ϕ -function has a maximum value of unity when the scale separation is zero.

We shall test the index of similarity on a set of ten statements of opinions selected at random from different parts of an attitude scale of 45 such statements. The random set of ten statements is less unwieldy to handle for illustrative purposes than the whole list of 45 in the church scale because the index involves a comparison of each statement with every other statement in the whole list. There will be therefore $10 \cdot 9 \cdot \frac{1}{2} = 45$ comparisons for a set of 10 statements while there would be $45 \cdot 44 \cdot \frac{1}{2} = 990$ comparisons necessary to handle the whole table of 45 statements.

The ten statements were selected so as to represent several degrees of attitude toward the church, including favorable, unfavorable, and indifferent opinions. Each statement is identified by a code number as follows:

2. I feel the church services give me inspiration and help to live up to my best during the following week.
4. I find the services of the church both restful and inspiring.
6. I believe in what the church teaches but with mental reservations.
11. I believe church membership is almost essential to living life at its best.
15. Sometimes I feel that the church and religion are necessary and sometimes I doubt it.
32. I believe in sincerity and goodness without any church ceremonies.
34. I think the organized church is an enemy of science and truth.
35. I believe the church is losing ground as education advances.
41. I think the church seeks to impose a lot of worn-out dogmas and medieval superstitions.
43. I like the ceremonies of my church but do not miss them much when I stay away.

In Table 1 we have all the necessary raw data. There are three types of fact here recorded: (1) The total number of individuals who endorsed each of the ten statements. These

TABLE I

NUMBER OF DOUBLE ENDORSEMENTS FOR ALL PAIRS OF OPINIONS

	11	2	4	6	43	15	32	35	41	34
11	454	334	349	209	60	66	64	85	19	15
2	334	573	492	263	72	85	81	88	23	15
4	349	492	696	354	127	163	148	152	26	28
6	209	263	354	620	152	221	220	225	67	54
43	60	72	127	152	265	122	116	116	46	33
15	66	85	163	221	122	407	207	214	72	67
32	64	81	148	220	116	207	546	315	142	136
35	85	88	152	225	116	214	315	548	165	166
41	19	23	26	67	46	72	142	165	224	123
34	15	15	28	54	33	67	136	166	123	219
<i>p</i>	.57	.72	.80	.57	.31	.43	.63	.61	.60	.71

are found in the diagonal of the table. For example, there were 696 individuals who endorsed statement 4 in the total group of about 1,500 persons who filled in the complete attitude scale. (2) The total number of individuals who endorsed any particular statement and any other particular statement. These data are found in the body of the table. For example, there were 263 individuals who endorsed both statements 6 and 2. (3) The reliability of each of the ten statements. These are found in the last row of the table. They were determined by the second method described above which was applied to each of the statements in the whole scale of forty-five opinions. For example, the reliability of statement 32 is .63 which means that it was endorsed by 63% of the estimated number of people who should have endorsed it if the statement had been perfect and if the subjects had been perfect in their reading and endorsing.

In Table 2 we have listed the ϕ -values for all the comparisons. This is done by equation 18. The following is an example of the calculation of the ϕ -coefficient for the two statements 4 and 32 with data from Table 1.

$$\phi_{4.32} = \frac{148}{\sqrt{.80 \times .63 \times 696 \times 546}} = .34.$$

It is seen that the table is symmetrical. The entries along

the diagonal are necessarily unity because there is of course no scale separation between a statement and itself. We shall now use these ϕ -values to measure the scale separation between all pairs of statements. This is done by entering an ordinary probability table with the values of ϕ in order to ascertain the deviation from the mean in terms of the standard

TABLE 2
THE ϕ -COEFFICIENTS OF SIMILARITY FOR ALL PAIRS OF OPINIONS

	11	2	4	6	43	15	32	35	41	34
11	1.00	1.02	.92	.69	.41	.31	.21	.29	.10	.07
2	1.02	1.00	1.03	.69	.39	.32	.22	.24	.10	.06
4	.92	1.03	1.00	.80	.59	.52	.34	.35	.10	.10
6	.69	.69	.80	1.00	.89	.89	.63	.65	.31	.23
43	.41	.39	.59	.89	1.00	1.02	.69	.70	.44	.29
15	.31	.32	.52	.89	1.02	1.00	.85	.89	.47	.41
32	.21	.22	.34	.63	.69	.85	1.00	.93	.66	.59
35	.29	.24	.35	.65	.70	.89	.93	1.00	.78	.73
41	.10	.10	.10	.31	.44	.47	.66	.78	1.00	.85
34	.07	.06	.10	.23	.29	.41	.59	.73	.85	1.00

deviation of the assumed Gaussian function. Each of these deviations will be regarded tentatively as the scale separation between the two statements concerned. When the value of ϕ is small we shall therefore assign a rather large separation to the two statements. When the value of ϕ is high, near unity, we shall assign a rather small scale separation to the statements. It is more convenient for this problem to use a probability table in which the maximum ordinate is unity than to use a table in which the total area is unity so that the maximum ordinate is .4. It is also more convenient to use a probability table which is entered with the ordinate to ascertain the deviation rather than to use a table which is entered with deviations or proportions to ascertain the ordinates. The latter kind of probability table requires interpolation for this problem. The separation between statements 43 and 6 may be taken as an example. The ϕ -coefficient for these two statements is .89 as shown in Table 2. With this ordinate of the probability curve, the deviation is $.48\sigma$ as recorded in Table 3.

The sign of the deviation is determined by the end of the scale which is arbitrarily called positive. In the present case the origin was arbitrarily placed at the opinion least favorable to the church namely statement 34. Therefore the statements favorable to the church are arbitrarily called positive with regard to the statements that are unfavorable to the church. It is entirely immaterial for scaling purposes which ends of the sequence of opinions are designated as positive and negative.

TABLE 3

EXPERIMENTAL SCALE SEPARATIONS BETWEEN ALL PAIRS OF OPINIONS. ($S_{top} - S_{side}$)

	11	2	4	6	43	15	32	35	41	34
11	.00	.00	-.41	-.86	-1.34	-1.53	-1.77	-1.57	—	—
2	.00	.00	.00	-.86	-1.37	-1.51	-1.74	-1.69	—	—
4	.41	.00	.00	-.67	-1.03	-1.14	-1.47	-1.45	—	—
6	.86	.86	.67	.00	-.48	-.48	-.96	-.93	-1.53	-1.71
43	1.34	1.37	1.03	.48	.00	.00	-.86	-.84	-1.28	-1.57
15	1.53	1.51	1.14	.48	.00	.00	-.57	-.48	-1.23	-1.34
32	1.77	1.74	1.47	.96	.86	.57	.00	-.38	-.91	-1.03
35	1.57	1.69	1.45	.93	.84	.48	.38	.00	-.70	-.79
41	—	—	—	1.53	1.28	1.23	.91	.70	.00	-.57
34	—	—	—	1.71	1.57	1.34	1.03	.79	.57	.00

The signs in Table 3 are recorded so as to show ($S_{top} - S_{side}$). For example, the scale separation ($S_{43} - S_6$) is found at the intersection of 43 at the top with 6 at the side. It is $-.48\sigma$. Similarly the separation ($S_6 - S_{43}$) is found at the intersection of 6 at the top with 43 at the side. It is $+.48\sigma$. The two halves of the table are symmetrical about the diagonal of zero entries. The ten statements were arranged in Table 1 in order of scale values determined by the method of equal appearing intervals.² All separations as large as 2σ or larger were ignored in Table 3 because when the separations become as large as that their reliabilities become too low to be acceptable. It is entirely arbitrary at what limit we shall drop the separations. They might be extended indefinitely if the

² A monograph 'The Measurement of Attitude Toward the Church' by Thurstone and Chave, not yet published. This monograph describes the construction and use of a scale of 45 opinions about the church and the distributions of attitude in several large groups.

observations were weighted but that is too awkward. In these tables we have recorded separations only as large as 2σ . There may also be some uncertainty as to how far the Gaussian curve can be used for the function $\phi = f(s)$ and this is another reason for not using scale separations larger than about 2σ .

We are now ready to determine the average scale separation between successive statements in the present list of ten. It is done as follows:

Let S_1 and S_2 be the scale values of any two statements whose separation is to be measured. Then

$$x_{12} = S_1 - S_2 \quad (27)$$

is a direct measurement of this separation which is obtained by the index of similarity ϕ_{12} . This index is in turn a function of the raw data $n_1, n_2, n_{12}, p_1, p_2$, so that

$$\phi_{12} = \frac{n_{12}}{\sqrt{n_1 n_2 p_1 p_2}} = \frac{1}{\sqrt{2\pi} \sigma} e^{\frac{-(s_1 - s_2)^2}{2\sigma^2}} = \frac{1}{\sqrt{2\pi} \sigma} e^{\frac{-x_{12}^2}{2\sigma^2}}. \quad (28)$$

But we also have many indirect measurements of x_{12} which may be shown as follows.

Let S_k be the scale value of any other statement except 1 and 2. Then

$$\begin{aligned} S_1 - S_k &= x_{1k}, \\ S_2 - S_k &= x_{2k}, \end{aligned}$$

so that

$$S_1 - S_2 = x_{1k} - x_{2k}, \quad (29)$$

and hence

$$S_1 - S_2 = \frac{1}{n} (\sum x_{1k} - \sum x_{2k}). \quad (30)$$

This equation is more accurate than (27) because it makes use of all the data in Table 1 while equation (27) makes use of only one of the ϕ -coefficients. Applying equation (30) to the data of Table 3 where n is in each of the nine successive comparisons the number of paired values, we obtain the successive scale separations shown in Table 4. We set the origin arbitrarily at statement 34 so that the final scale values from this

origin are as shown in Table 4. For example, the final scale separation between opinions 4 and 6 is obtained by equation 30. There are eight paired values for these two opinions in Table 3. The numerical values are as follows

$$\begin{aligned}\sum x_{4k} &= +5.35, & n &= 8, \\ \sum x_{6k} &= +0.46, & S_4 - S_6 &= +0.6113.\end{aligned}$$

Note that the sum $\sum x_{6k}$ takes a different value when equation 30 is used to determine the scale separation between opinions 6 and 43 because there are then available ten paired values instead of eight for the interval 4 to 6.

TABLE 4

34	0.0000
	<u>.2757</u>
41	.2757
	<u>.5629</u>
35	.8386
	<u>.0800</u>
32	.9186
	<u>.4010</u>
15	1.3196
	<u>.1370</u>
43	1.4566
	<u>.3370</u>
6	1.7936
	<u>.6113</u>
4	2.4049
	<u>.2275</u>
2	2.6324
	<u>.0388</u>
11	2.6712

We now want to test the internal consistency of our calculations. On the basis of the final scale values of Table 4 we may construct a table of calculated scale separations. This has been done in Table 5. For example, the scale values of statements 43 and 35 are 1.46 and .84 respectively. Consequently the calculated scale separation ($S_{43} - S_{35}$) is +0.62 as recorded in Table 5, and the separation ($S_{35} - S_{43}$) is the same distance with sign reversed, namely -0.62, also recorded in the same table. The separations of Table 5 are based entirely on the ten scale values of Table 4.

TABLE 5

CALCULATED SCALE SEPARATIONS OF ALL PAIRS OF OPINIONS ($S_{\text{top}} - S_{\text{side}}$)

	11	2	4	6	43	15	32	35	41	34	Σ
11	.00	-.04	-.27	-.88	-1.21	-1.35	-1.75	-1.83	-2.40	-2.67	-12.40
2	.04	.00	-.23	-.84	-1.18	-1.31	-1.71	-1.79	-2.36	-2.63	-12.01
4	.27	.23	.00	-.61	-.95	-1.09	-1.49	-1.57	-2.13	-2.40	-9.74
6	.88	.84	.61	.00	-.34	-.47	-.88	-.96	-1.52	-1.79	-3.63
43	1.21	1.18	.95	.34	.00	-.14	-.54	-.62	-1.18	-1.46	-.26
15	1.35	1.31	1.09	.47	.14	.00	-.40	-.48	-1.04	-1.32	1.12
32	1.75	1.71	1.49	.88	.54	.40	.00	-.08	-.64	-.92	5.13
35	1.83	1.79	1.57	.96	.62	.48	.08	.00	-.56	-.84	5.93
41	2.40	2.36	2.13	1.52	1.18	1.04	.64	.56	.00	-.28	11.55
34	2.67	2.63	2.40	1.79	1.46	1.32	.92	.84	.28	.00	14.31
Σ	12.40	12.01	9.74	3.63	.26	-1.12	-5.13	-5.93	-11.55	-14.31	0.00

Now we want to know how closely these calculated scale separations of Table 5, based on the ten scale values, agree with the 45 experimentally independent scale separations of Table 3. The discrepancies between Tables 3 and 5 are listed individually in Table 6. The discrepancies between the

TABLE 6

DISCREPANCIES BETWEEN EXPERIMENTAL AND CALCULATED SCALE SEPARATIONS

	11	2	4	6	43	15	32	35	41	34	Σ
11	.00	.04	-.14	.02	-.13	-.18	-.02	.26	—	—	-.15
2	-.04	.00	.23	-.02	-.19	-.20	-.03	.10	—	—	-.15
4	.14	-.23	.00	-.06	-.08	-.05	.02	.12	—	—	-.14
6	-.02	.02	.06	.00	-.14	-.01	-.08	.03	-.01	.08	-.07
43	.13	.19	.08	.14	.00	.14	-.32	-.22	-.10	-.11	-.07
15	.18	.20	.05	.01	-.14	.00	-.17	.00	-.19	-.02	-.08
32	.02	.03	-.02	.08	.32	.17	.00	-.30	-.27	-.11	-.08
35	-.26	-.10	-.12	-.03	.22	.00	.30	.00	-.14	.05	-.08
41	—	—	—	.01	.10	.19	.27	.14	.00	-.29	.42
34	—	—	—	-.08	.11	.02	.11	-.05	.29	.00	.40
Σ	.15	.15	.14	.07	.07	.08	.08	.08	-.42	-.40	.000

$$\text{Average discrepancy} = \frac{\Sigma |x_e - x_c|}{N} = \frac{9.34}{88} = 0.106$$

experimental and the calculated scale separations in Table 6 vary between zero and $.32\sigma$ with a mean discrepancy of only $.106\sigma$. This mean discrepancy is only about 1-25 or 4 per cent of the range of the scale values, 2.67σ , for the ten statements.

Another set of ten statements, also selected at random from the entire list, has been subjected to the same analysis with comparable results.

The question might be raised why we have not used correlational coefficients instead of the ϕ -coefficient here described. Dissimilarity can of course be indicated merely by a correlational index or by contingency methods. Such indices do not constitute measurement except by a generous interpretation of the word measurement. We have attempted truly to measure degree of functional dissimilarity of two attributes or reactions. In order to satisfy what seems to be a fundamental requirement of measurement it is reasonable to expect that if the difference between two entities *a* and *b* is, let us say, plus five units, and if the difference between two entities *b* and *c* is, let us say, plus three units, then the difference between the two entities *a* and *c* should be the sum of these two differences, namely, plus eight units, if all three quantities really measure the same attribute.

This simple requirement is not satisfied by correlational coefficients. If the correlation between *a* and *b* is .80 and that of *b* and *c* is .40 it does not follow that the correlation between *a* and *c* is some additive function of these coefficients. We have postulated a continuum, the attitude scale, and we want to measure separations between points on this continuum so that our measurements are internally consistent; so that $(a - b) + (b - c) = (a - c)$ but such consistency is not found by correlational procedures.

Let it be desired to measure the areas of a lot of circles. Let the diameter of each circle be used as an index of area. It is now possible to arrange the circles in rank order according to area by means of the diameter-index. It is also possible to say of two circles that they must have the same areas because their diameters are equal, but these diameter measurements are hardly to be called measurements of areas. Equal increments of the diameter-index do not correspond to equal increments of what we set out to measure, namely area. The unit of measurement of the diameter does not correspond to a constant increment of area. All of this is childish simple

but the reasoning is the same as regards correlational coefficients. They are not measures of dissimilarity. They are merely numerical indices of dissimilarity. In fact, correlation coefficients are what one resorts to in the absence of hypothesis and rational formulation. If the problem admits of rational formulation, then that function should be written and tested directly by experiment. If the problem is so complex that it defies analysis we can still correlate the variables and represent by correlation coefficients the degree of association between them. That is better than nothing, but it is not really measurement by our simple criteria. These considerations have led me to regard correlation coefficients as symbols of defeat. They constitute a challenge to try again and to outgrow the necessity for using them.

My efforts recently in psychological measurement have been to define in every case a continuum, to allocate people, tasks, and other entities to the continuum under investigation, and to check its validity by the simple criteria that have just been described. I believe that such efforts will prove more fruitful for psychological theory than merely to correlate everything with everything else under heaven.

The results of our attempt to construct an attitude continuum are shown graphically in Fig. 2, in which the ten



FIG. 2.

opinions are shown with their allocations to the attitude scale. An actual scale for measuring attitude should contain many more opinions and they should be so selected that they constitute as far as possible an evenly graduated scale. The church scale previously referred to has 45 opinions which have been selected from a list of 130 so as to constitute an evenly graduated scale. Our present purpose has been to show how the *method of similar reactions* enables us to construct such a scale from the records of endorsements. It is hoped that the

method may also prove useful as an objective test for the validity of other concepts such as extroversion-introversion, ascendance-submission, and the like.

SUMMARY

We have developed a new psychophysical method for measuring the psychological dissimilarity of attributes. This method assumes that if two attributes tend to coexist in the same individual they are regarded as functionally similar while if they are more or less mutually exclusive so that they tend not to coexist in the same individual, then they are functionally dissimilar. The degree of similarity is measured in terms of the ϕ -coefficient which enables us to allocate the attributes along a single continuum, and to measure the degree of similarity by scale separations on this continuum or scale. The method may be called a *method of similar attributes* or a *method of similar reactions*.

The ϕ -coefficient enables us to ascertain whether a series of attributes really belong functionally on the same continuum. This is done by the test of internal consistency as shown in Table 6. The method has been applied to the record of endorsements of 1500 people to ten statements of opinion about the church. It has been shown that these opinions can be allocated to a single continuum with measured scale separations. It has been the purpose of this study to make a rational formulation for the association of attributes by which the existence of continuity in a series of attributes may be experimentally established and by which their functional dissimilarities, the scale separations, may be truly measured. For these purposes correlational procedures are inadequate because correlational coefficients are not measurements.

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EMOTION AND THE INCIDENCE OF DISEASE:
THE INFLUENCE OF THE NUMBER OF
DISEASES, AND OF THE AGE AT
WHICH THEY OCCUR

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Since the publication of a preliminary paper upon the relation between emotional response and the personal history with respect to disease,¹ the writer has extended his study so that it now includes a larger number of persons and enables one not only to reexamine in the light of new evidence the former indications, but also to enquire into matters not considered in the earlier report.

The method of obtaining records of emotional responses and of giving quantitative expression to them, and of arriving at the personal history of disease, together with a statement of what is meant, precisely, by the term 'disease,'—all these things have been set forth in detail elsewhere,² and need now only be sketched;³ it will be understood that they hold also for the study here reported. The computation of the tables

¹ 'Emotion and the Incidence of Disease,' *J. Abn. & Soc. Psychol.*, 1926, 21, 19.

² 'Anger and Fear: their Probable Relation to Each Other, to Intellectual Work, and to Primogeniture,' *Amer. J. Psychol.*, 1927, 39, 125-140.

³ The histories of disease came from the persons' medical examination upon entrance to college; and the diseases included were: appendicitis, asthma, Bright's disease, constipation, diphtheria, encephalitis, headache, heart trouble, infantile paralysis, infections (inflammation of the bowels, discharging ear, etc.), influenza, malaria, meningitis, neurasthenia and nervousness, pleurisy, pneumonia, rheumatism, scarlet fever, smallpox, St. Vitus' dance, tonsillitis, tuberculosis, and typhoid fever.

The scores for fear and for anger were derived from the reports by students each of whom noted his own behavior in certain actual situations. He was given a printed description of about twenty possible fear-situations, and a description of six possible fear-reactions to each of these situations; and he was requested to record, upon the day of each occurrence itself, which of the described reactions his own actual response in each case most resembled. These observations were made by each person during three days for fear; and—but always separated from the fear-observations by an interval of several weeks—for a like three days for anger.

of the present paper are the work chiefly of Dr. Yoshioka and Miss Glines.

I

Returning for a moment to a question hesitatingly answered before—whether persons with a history of disease give an emotional response different from that of persons whose history is without disease—the enlargement of data for a new answer is as follows. Where earlier there were 650 persons whose fear-reactions could be compared with their personal histories with respect to disease, there are now—including the earlier number—1,000 persons. And where the anger-reactions and the histories of about 900 persons were at hand, these are now gathered—and again including the earlier number—for more than 1,600 persons. At present it is possible also to give a more exact quantitative expression for several of the relations which before were stated only in vague terms of more or less.

In summary the evidence now at hand is given in Tables 1 and 2.

TABLE 1
FEAR, AND THE OCCURRENCE OF DISEASE GENERALLY

	No. of Persons whose Scores for Emotion were Used	No. of Reactions upon which their Scores were Based	Mean Score and its Probable Error for the Group	Diff. and P.E. of the Diff. bet. the Means	Diff. Divided by its P.E.
Persons whose history involves disease.....	689	1,479	100.40±0.48	1.04±0.80	1.30
Persons whose history is without disease.....	318	702	99.36±0.68		

TABLE 2
ANGER AND THE OCCURRENCE OF DISEASE GENERALLY

	No. of Persons whose Scores for Emotion were Used	No. of Reactions upon which their Scores were Based	Mean Score and its Probable Error for the Group	Diff. and P.E. of the Diff. bet. the Means	Diff. Divided by its P.E.
Persons whose history involves disease.....	1,088	2,855	101.28±0.32	3.92±0.56	7.00
Persons whose history is without disease.....	535	1,396	97.36±0.44		

It is probable—so the evidence here indicates—that among persons whose histories involve disease, the average response in situations which call forth fear will be more intense than that of persons whose histories are without disease—probable, that is, to a degree represented by the ratio of 81 to 100.

But with regard to anger the probability of an intenser average response in a group of persons whose histories involve disease than in a group every member of which has been free from disease is very high indeed, being higher than is expressed by the ratio of 9,999 to 10,000.

These findings confirm those of the earlier report already cited, as to the relation between disease and emotional response. Here, as before, there appears an intenser emotion in those who have suffered disease—a relation more evident with respect to anger than with respect to fear. And beyond the earlier report, the present is based upon a much larger collection of observations and is expressed in more precise quantitative terms.

II

Passing now to a topic which lay beyond the scope of the earlier report, let us enquire whether emotional reactions have any discernible connection with the *number* of diseases to which an individual has been subject—not the number of times a person may have been sick with the same disease, upon which point the records available were silent, but the number of different diseases which he had, regardless of the frequency of any one of them.

Tables 3, 4 and 5 give, in summary, the evidence with regard to fear.

TABLE 3

FEAR, AND THE NUMBER OF DISEASES TO WHICH A PERSON HAS BEEN SUBJECT

No. of Diseases Reported	No. of Persons Reporting the Particular Number of Diseases	No. of Emotional Reactions upon which their Scores were Based	Mean Score and its P.E., for the Group
0.....	318	702	99.36±0.68
1.....	172	390	102.36±1.29
2.....	157	332	96.96±1.34
3.....	90	190	100.48±1.48
4.....	49	101	101.16±1.58
5 or more.....	30	81	105.32±1.71

TABLE 4

FEAR, AND THE NUMBER OF DISEASES TO WHICH A PERSON HAS BEEN SUBJECT
*Difference and Probable Error of the Difference, between the Mean Emotion-Scores of the
 Groups of Persons given in Table 3*

No. of Diseases	1	2	3	4	5 or more
0.....	+3.00±1.46	-2.40±1.51	+1.12±1.62	+1.80±1.72	+5.96±1.84
1.....		-5.40±1.86	-1.88±1.96	-1.20±2.04	+2.96±2.14
2.....			+3.52±2.00	+4.20±2.08	+8.36±2.18
3.....				+0.68±2.16	+4.84±2.26
4.....					+4.16±2.33

TABLE 5

FEAR, AND THE NUMBER OF DISEASES TO WHICH A PERSON HAS BEEN SUBJECT
*Values Obtained by Dividing Each Difference by the Probable Error of that Difference
 in Table 4*

No. of Diseases	1	2	3	4	5 or more
0.....	2.06	1.59	0.69	1.04	3.23
1.....		2.90	0.96	0.59	1.38
2.....			1.76	2.02	3.84
3.....				0.31	2.14
4.....					1.78

From these tables it appears that there is a step—namely, from one disease to two diseases—which seems to bring a decrease in the emotional response. But after reaching two diseases, we find regularly a slight increase in emotional response with each increase in the number of diseases. That is to say, fear seems intensified as we pass from a lower to a higher incidence of disease. From an index number of approximately 97 for two diseases, there is a rise to an index number of somewhat over 105 for five or more diseases. The increment in mean emotion-score for three diseases, as compared with two, is 3.52; for four diseases, as compared with two, it is 4.20; for five or more diseases, as compared with two, it is 8.36.

Yet nearly all these progressions of the emotional response in the case of fear are of doubtful significance. In only a single instance is there a difference which is more than three times

the probable error of the difference—namely, between those who had two diseases and those who had five or more diseases. Here the difference is 3.84 times its probable error.

With respect to anger the evidence which bears upon the same question is summed up in Tables 6, 7, and 8.

TABLE 6

ANGER, AND THE NUMBER OF DISEASES TO WHICH A PERSON HAS BEEN SUBJECT

No. of Diseases Reported	No. of Persons Reporting the Particular Number of Diseases	No. of Emotional Reactions upon which their Scores were Based	Mean Score and its P.E., for the Group
0.....	535	1,396	97.36±0.44
1.....	274	934	99.80±0.67
2.....	264	951	99.44±0.69
3.....	140	552	103.52±0.64
4.....	67	272	104.36±0.86
5 or more.....	44	201	107.12±1.09

TABLE 7

ANGER, AND THE NUMBER OF DISEASES TO WHICH A PERSON HAS BEEN SUBJECT
Difference and Probable Error of the Difference, between the Mean Emotion-Scores of the Groups of Persons Given in Table 6

No. of Diseases	1	2	3	4	5 or more
0.....	+2.44±0.81	+2.08±0.82	+6.16±0.78	+7.00±0.96	+9.76±1.72
1.....		-0.36±0.96	+3.72±0.93	+4.56±1.09	+7.32±1.28
2.....			+4.08±0.85	+4.92±1.10	+7.68±1.29
3.....				+0.84±1.07	+3.60±1.26
4.....					+2.76±1.38

TABLE 8

ANGER, AND THE NUMBER OF DISEASES TO WHICH A PERSON HAS BEEN SUBJECT
Values Obtained by Dividing Each Difference by the Probable Error of that Difference, in Table 7

No of Diseases	1	2	3	4	5 or more
0.....	3.02	2.55	7.94	7.26	8.33
1.....		0.38	4.01	4.19	5.72
2.....			4.79	4.47	5.96
3.....				0.79	2.85
4.....					1.99

These three tables indicate that in general the number of diseases befalling an individual is connected with the intensity of his anger-response; the greater the number of diseases incurred, the stronger are his anger-reactions.

The course which the numbers run in these tables is not, however, entirely uniform. There is a reversal of its general direction as we pass from the group of persons who had but one disease to the group with two diseases; in this particular region, the mean score of those with the greater number of diseases is slightly less than the mean score for those with the smaller number. But this single departure from the general trend is of small amount and of relatively large probable error, and apparently is accidental. In all other cases the mean emotion-score for any group is greater than that of any other group having a smaller number of diseases. When the number of diseases is increased by but one, we usually find an increase in the anger-response; but the increase in the emotional score may be small and of relatively large probable error. But when we pass from a given group to another group whose number of diseases is greater by two or more, significant differences are more frequently found. The maximum and most significant effect appears when we compare the groups having one or two diseases with the group having five or more diseases: the anger of this latter group rises seven points, and this increase is more than five times the probable error of the increase.

It would seem highly probable, then, that there is a difference between the anger-responses of those who have had few diseases and those who have many. With a greater tendency to disease, there seems to go an increment of irascibility. The threshold for anger-stimuli is lowered in those who have less successfully resisted morbid attack.

In anger, then, as in fear—but far less doubtfully in anger—there is evidence of a positive connection between the strength of the emotional response and the number of different diseases which a person has incurred. The more diseases the more intense the emotion of the kinds here considered. The person who has had a greater variety of illnesses is ready to

respond more pronouncedly to emotional situations, especially to those likely to evoke anger.

III

Further, let us enquire whether the person's age when he suffers disease is significant for his later emotional life.

If one takes those individuals whose history is of disease, and then divides them into (a) those whose diseases were reported as occurring only in the first six years of age, and (b) those whose diseases occurred only at a later age, the outcome of comparing these two groups is stated in Tables 9 and 10.

TABLE 9

FEAR, AND THE AGE WHEN DISEASE OCCURRED

	No. of Persons	No. of Emotional Reactions upon which their Scores were Based	Mean Score and its Probable Error, for the Group	Diff. and P.E. of the Diff. bet. the Means	Diff. Divided by its P.E.
With disease occurring only in the first six years of age.....	34	51	97.88±2.76	2.04±2.84	0.72
With disease occurring only at a later age.....	337	752	99.92±0.66		

TABLE 10

ANGER, AND THE AGE WHEN DISEASE OCCURRED

	No. of Persons	No. of Emotional Reactions upon which their Scores were Based	Mean Score and its Probable Error, for the Group	Diff. and P.E. of the Diff. bet. the Means	Diff. Divided by its P.E.
With disease occurring only in the first six years of age.....	50	219	105.36±0.94	5.56±1.00	5.56
With disease occurring only at a later age.....	512	1,828	99.80±0.33		

In regard to fear, the evidence is quite inconclusive. What slight difference there is between those of earlier and those of

later morbidity is in the direction of a greater responsiveness to fear-situations in persons whose diseases came after the first six years of age. But the probable error of this difference is larger than the difference itself, so that the least of weight can be given to this indication.

TABLE II

FEAR, AND THE AGE WHEN DISEASE OCCURRED

Years of Age, Inclusive, within which All the Individual's Diseases Occurred	No. of Persons	No. of Emotional Reactions upon which their Scores were Based	Mean Score and its Probable Error, for the Group	Diff. and P.E. of the Diff. bet. the Means	Diff. Divided by its P.E.
0-5.....	34	51	97.88 \pm 2.76	-0.24 \pm 3.16	0.08
6-10.....	60	140	97.64 \pm 1.54	+6.04 \pm 2.12	2.85
11-15.....	76	156	103.68 \pm 1.46	-3.48 \pm 2.28	1.53
16 and over....	42	92	100.20 \pm 1.74		

TABLE 12

ANGER, AND THE AGE WHEN DISEASE OCCURRED

Years of Age, Inclusive, within which All the Individual's Diseases Occurred	No. of Persons	No. of Emotional Reactions upon which their Scores were Based	Mean Score and its Probable Error, for the Group	Diff. and P.E. of the Diff. bet. the Means	Diff. Divided by its P.E.
0-5.....	50	219	105.36 \pm 0.94	-5.60 \pm 1.24	4.52
6-10.....	102	349	99.76 \pm 0.79	-1.64 \pm 1.08	1.52
11-15.....	114	424	98.12 \pm 0.73	+0.96 \pm 1.32	0.73
16 and over....	63	164	99.08 \pm 1.10		

As for anger, it is fairly clear that the persons whose diseases occurred only in early childhood had in their later life stronger anger-reactions than had the persons whose illnesses all came later. The difference in strength of emotion connected with the two periods of morbidity is not only relatively large in comparison with the general run of differences appearing in these studies, but it is relatively trustworthy, being more than five times its probable error.

When we ask whether any significant differences appear when we break up the group of those whose diseases occurred after five years of age—that is to say, break the group into sub-groups according as their diseases occurred wholly in the ages from 6 to 10, inclusive, or 11 to 15, or 16 and over—while repeating for comparison what has already appeared for those who were under 6 years, the facts are stated in Tables 11 and 12.

With respect to fear, no indubitably significant difference appears as we pass from group to group. The nearest approach to such a difference is observed in the transition from those whose ages ranged between 0 and 10 years, inclusive, to those whose ages ranged between 11 and 15 years, inclusive; for there is an indication that a disease in the latter period is accompanied by an increased tendency to fear. But the relatively large probable error of the difference leaves some doubt as to the meaning of the indication, although the probability that the difference is significant is as 97 to 100.

For anger, however, there is a fairly clear drop in the intensity of the emotional response as we pass from the group of the youngest to the group of those next of age—a drop which is not repeated in the transition between any other adjacent groups. The unique difference between those of the earliest age and those of the next older age is of more than five points, and is more than four times its probable error. It is highly probable, then,—to a degree expressed by the ratio of 999 to 1000—that disease at this earliest period is of greater significance for anger-reactions than is disease at any of the other periods of life here considered. If this finding should be confirmed, it might be of practical importance in estimating the significance of health and disease in early childhood for the later nervous and mental constitution.

IV

Finally one might review some of the features now before us.

The indication of a bond between emotion and the several aspects of disease here considered we have seen to be more

pronounced for anger than for fear. And a similar advantage of anger over fear was seen at least once in the report last cited,⁴ where anger appeared more closely connected with primogeniture than is fear, although anger and fear seem to be about equally related to intellectual work.

Should we infer in the present instance that anger is, in point of fact, more intimately related to disease than is fear? Such a difference of connection may exist, but one might well hesitate to believe it from the present evidence. For in the method used, the mesh may have been of a size to catch important facts of anger, while allowing those of fear to slip through. And the truth then would be, not that disease is of less significance for our fears, but that we have failed to obtain evidence for such relations as exist. Failure to obtain does not prove that there is nothing to obtain. One might well believe that the method here employed gives a more satisfactory index of anger-tendencies than of fear-tendencies. In the everyday life of the individual lapped in civilization, there seem clearly to be more occasions for anger—even if it be only a mild unspoken 'irritation'—than for fear. A smaller number of my observers, in a duration of time equal to that allotted for noticing anger, had anything to report regarding their fears. And not only did fewer persons report, although a like number were asked to report; but of these who actually reported, each person told, in the average, of fewer fear-situations. It may well be that my procedure did little more than break the surface of fear, while reaching a less superficial layer of anger.

For this reason I question whether the greater uncertainty in the tables regarding fear may not be due, in part at least, to a less successful approach to the reactions of fear. A more refined method might reveal an equal connection running from the two emotions. But even though the strength of the connection should in the end prove to be different, yet the present evidence suggests that the relation between disease and fear, so far as any relation appears at all, is usually of a like kind to that between disease and anger. In the case

⁴ *Amer. J. Psychol.*, 1927, 39, 138ff.

both of anger and of fear the emotional response where there is a history of disease is heightened beyond what occurs without such a history; in the case both of anger and of fear the response where there have been more diseases is heightened beyond what occurs with fewer diseases. Such findings, together with the finding in regard to emotion and intellectual work, are consonant with the indication of an earlier report already cited,⁵ that in the same person the strength of anger and the strength of fear are correlated positively. That diseases in early childhood may have with fear a connection opposite to the connection they have with anger—this is quite in keeping with the low degree of this positive correlation.

As to the precise character of the connection between emotion and disease—the occurrence of disease in general, the number of diseases incurred, and the age at which they occurred—the present study leaves us uncertain. There are several forms which the causal bond might take, while remaining within the limits set by the evidence.

1. The two groups of factors—the emotion-tendencies and the disease-tendencies—may be common symptoms of a constitution not only more responsive to situations of anger and probably of fear, but also less resistant to various diseases and especially to disease in childhood.

2. Disease by its very occurrence, and especially if early and frequent, may leave a lasting—even though slight—trace in the neural, glandular, and psychic constitution such as to cause a tendency to more intense anger and probably also to fear.

3. Anger and also to some extent fear by their very occurrence, especially if habitual and intense, may tend to alter the psychic, neural, and glandular constitution so that there is slightly less resistance to various diseases, especially in early childhood.

4. The causal relation may run in more than one of these directions at once; so that any two or perhaps all three of the preceding propositions may be true, and each have its own degree of importance.

⁵ *Amer. J. Psychol.*, 1927, 39, 134ff.

The present evidence gives no clue by which we may intelligently select from among these possibilities. Each of us will incline to one or another, according to his mental habit and predilection. Later investigation alone can decide the truth.

But whatever be the more correct interpretation of the present indications, these indications themselves may well be re-stated in brief.

1. The principal finding in an earlier report—namely, that persons who at any time have been subject to disease tend in later life to respond more intensely to anger-situations, and probably also to fear-situations, than do persons who have always been free from serious disease—this, in the light of wider evidence, still appears to be true.

2. Of persons alike in having had disease, those who have had a greater range of diseases probably tend to respond more intensely to fear situations than do those who have had a lesser range. In the case of anger the probability that an incidence of more diseases tends to be connected with more intense emotional responses seems fairly high.

3. The period of life at which our diseases occur is probably important for our emotions. With respect to fear the indications are unclear; but such as they are, they point to a greater significance in diseases occurring in the period from eleven to fifteen years, inclusive, than in either of the periods earlier. For anger, the evidence seems clear that persons subject to disease in the first six years of childhood tend to more intense emotion than do those whose diseases occurred in any of the later periods here considered.

[MS. received December 24, 1928]

DISCUSSION

SOME SUCCOR FOR PROFESSOR KUO

Professor Kuo's paper 'Purpose and the Trial Error Fallacy' (PSYCHOL. REV., 1928, 35, pp. 414-433) seems to have raised such a storm of protest from Tolman, Roberts, and Rosenow, in the November 1928 issue of the Review that some support for Kuo seems timely. Professor Kuo himself is perfectly capable of dealing with his critics and I enter the discussion only as an interested reader.

Professor Tolman in reviewing the work of Blodgett, Adams, and Elliott states: "From these facts I would conclude that the sort of movements released by hunger contractions are contingent, not only upon these hunger contractions themselves and upon the external stimuli coming from the maze alleys *per se*, but also upon the characters of the end-situation which the animals upon *preceding trials* [italics mine] have found to result." The question I raise is this, 'Is an end-situation which has *occurred* still an end-situation?' It is of course classified as an end-situation by the experimenter, but for the animal it is *now* a new stimulating condition even 'prepotent' as Kuo maintains.

In describing the experiments cited, Tolman concludes, "(1) that the '*restless* movements' in the maze will change in character when a switch is made from no food at the end of the maze to food; (2) that they will likewise change when a switch is made from no food to a customary 'sign' for food *until further experience causes this sign to lose its signification* in this specific situation; and (3) that they will change their character when a switch is made from a previously given customary food to a new unfamiliar food" [italics mine]. For me these are perfectly adequate conclusions as they stand and need no further amplification. I think they would gain in clearness if the words that I have italicized were omitted. I do not see any advantage in selecting out these particular forms of reactions and classifying them as being, (or caused by) 'determining propensities, behavior drives, steers, tendencies, purposes.' In the experiments quoted all the conditions are stated that make it possible to modify the reactions of the rats or to predict the behavior

of other rats (within the limits of the experiment, of course). Why tack on the 'determining propensities,' etc., since any further control over behavior or greater accuracy in prediction must come from *other and more extensive experiments* anyway? We cannot develop better control and more accurate prediction from mere verbal classification.

Dr. Rosenow claims that he cannot get along without the concept of purpose. Of course if this is the case his argument is unanswerable. However, when we actually consider the illustration which he uses as a typical purposive act, namely, that of a man recovering his wind-blown hat, one wonders whether this action is purposive in the traditional sense of the term in that the future consequences of not running are the causes of the present running. The hat retrieving activity is objective in the sense that if I had asked a dozen bystanders 'Is the man running to recover the hat?' I should probably get the uniform answer 'yes' from all of them. However, had I asked the bystanders to indicate the *purpose* or *goal* back of the running, the uniformity among the bystanders would have been much less. Now if Dr. Rosenow limits his purposive behavior to the first class (that which is verifiable by others), it seems to me that all objections vanish and that he does not disagree with Kuo. This, however, does not settle the question as to whether the 'purposive psychologists' will accept Rosenow's limitations.

My objections to purposive concepts and even to such a concept as Kuo's 'prepotent' stimulus, rest upon the fact that these classifications can not be made uniform enough to be treated scientifically. If I were to ask a dozen psychologists to name the ten most important 'purposive actions' or the ten most 'prepotent' stimuli, each psychologist would hand in a different list, and if I then asked them to get together and make up a list upon which all would agree, not only would I fail to get such a list but I should probably find the disagreement greater than ever. This is why I object to the subjective or mentalistic categories of traditional psychology.

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